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COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE V.

Effects of carbonic acid on the blood of man and animals.—Modifications of the blood induced by temperature.— Experiments of Hales.—Illustrations.—Experiments of the lecturer.—Treatment of cholera.—Effects of injecting cold water into the veins.—Changes in the blood during chlorosis.—Obstacles to the study of the pathology of the blood.

GENTLEMEN:—We will commence our studies for to-day with a case of asphyxia, the details of which have been furnished me by M. James. You will find that the facts elicited by its examination afford a very curious confirmation of some of the principles we have already deduced from our experiments. The patient was a female suffocated by the fumes of charcoal; here is some of her blood, which, you observe, is quite fluid. I will not, for the present, examine into the cause of its fluidity; but the case gives us an opportunity of verifying in the human subject the mode in which such blood affects the condition of the organs. I have explained to you at length the serious consequences of non-coagulability. I have told you that, when so affected, the blood cannot traverse the capillary vessels, or, at least, that its circulation through them is materially modified; that it stagnates in the parenchyma of the organs, is extravasated, alters their appearance, and sometimes renders them utterly unfit for the performance of their several functions. For my part, whenever I meet with blood of this description, I feel certain that the lungs have been the seat either of engorgement, hepatisation, œdema, or apoplexy; and if its fluidity be still greater, I feel almost positive that there has been effusion into the cavity of the pleura. This shows you how important our investigations may become in a diagnostic point of view. By proceeding methodically, and not yielding to the numerous obstacles we encounter, we reap the satisfaction of gradually accumulating facts, the knowledge of which is calculated to advance the science of pathology. For example, it was formerly the habit to ascribe the various disorders I have just enumerated to a certain morbific matter borne along through our organs, and depositing in them the germ of irritation. This irritation was at first supposed to be slow in its progress, but subsequently to require full development, and give rise to an incomprehensible something termed inflammation. This latter was said to be complicated with the prostration or super-excitation of the vital properties, wherewith all the tissues of the economy had been bountifully supplied. But, Gentlemen, it has been shown to demonstration, that the empire of inflammation is even more widely extended that it was the habit to assume; and, what is remarkable, it is I who, like a generous enemy, have conferred on it powers of which even its very warmest partisans had deprived it. *They* limited its action to living organs; *I* have extended it to the tissues when they have ceased to live. Many and many a time have I proved, by experiment, that its most terrible symptoms develop themselves in parts *wholly inanimate*. See the absurd conclusions that are drawn, naturally enough, too, as far as the process of deduction goes, from the majority of the physiological and pathological doctrines of our time. Turn where they will our medical men find nothing but vitality, just as if the organisation of the human frame were not quite as worthy of our admiration, regulated by physical laws, as by a jumble of pretended vital properties. They will not allow a membrane to be a membrane; they refuse to believe that when a membrane is brought in contact with liquids, either imbibition or exhibition follows. No, that would be too simple; they must have exhalant vessels and absorbent vessels, and they must endow these vessels with intelligence, and make this intelligence decide on the fitness of this or that substance to enter

their cavity. Experimental inquiry gives the most striking refutation to all these reveries. But it has a hard task to effect the triumph of truth; so certain is it that a single fallacious idea, emitted by chance, may retard, for an indefinite period, the progress of the sciences. People are generally persuaded that physiology, as taught in the schools, has made extraordinary progress within the last thirty years; but a little reflection shows that unfortunately this is a complete mistake. What difference, in truth, is there between the archœus of Van Helmont, or the monstrous conceptions of Paracelsus, and the vital laws and properties admitted by our modern schools? I see no more than a change in words; the ideas remain unaltered; each man has done his best to show his talent, and make a name, by expounding these imaginary laws, but nothing more; and, assuredly, they might have chosen a better way to ensure a solid reputation than by battling and disputing about words; it would have been a surer and more simple plan to avow their ignorance, and seek in silence the causes of such phenomena as seemed inexplicable. But let us return to our subject, and examine the lungs of this woman, whose blood we found in a liquid state; the organ is, as you perceive, of a blackish-red colour, and on pressing it between the fingers only very slight crepitation is detected, an evident proof of its imperfect impermeability to the air. The colour of these brown spots resemble that of blood deprived of all contact with atmospheric oxygen. When I cut into the parenchyma of the organ, you see that black blood is extravasated into it in greater or less quantities, according to the part we examine. There are, no doubt, here and there, a few points of more florid tint, but that is the result of insufflation, as I shall presently explain to you. Thus, in the same manner as the altered appearance of the lung might have led us to anticipate the condition in which we found the blood, so the modification of that fluid perfectly accounts for the state of the lung. Here you see exemplified the close relation that exists between the organs and the liquid that circulates through them. The blood, from a certain cause, ceased to come in contact with atmospheric air, it in consequence almost immediately lost one or more of its normal properties, and the lung, in its turn, suffered from the modified characters of the blood. Stagnation and infiltration took place, and it is probable that, had not the duration of the disease been so limited, the disorders induced would have been still more serious.

Previous to the lecture, and without knowing that this case was about to be communicated to me, I had asphyxiated an animal with carbonic acid gas. This is a fortunate chance; we will now avail ourselves of it, by comparing the two cases, so as to discover their points of difference and resemblance.

In this animal, the subject of my experiment, death supervened in a few instants, the blood will, therefore, I doubt not, be but little altered.

[M. Magendie here commenced the autopsy.]

Here is the lung; the blood contained in its vesicles has exactly the same tint in every part of the organ as that of the woman of whom we have been speaking. When insufflation is practised you see that its whole surface assumes almost instantaneously a florid red colour. The reason of this phenomenon is simple; the blood had stagnated for a short while only in the pulmonary areolæ; hence it has not altogether lost its essential properties; it still retains that of reddening perfectly under the influence of the oxygen of the air. On the other hand, insufflation had little effect on the organs of the human subject, whose death was more lingering; the florid hue produced in them was comparatively pale, and existed only in isolated patches. This fact will throw some light on the study of organic lesions, for the different conditions of the blood may actually point out *the age of the disease*. You observed that it was completely liquefied in the woman suffocated by the vapours of burning charcoal, and, in consequence, there was considerable sanguineous infiltration of the pulmonary tissue; hence, I presume, that the sufferings of the poor victim must have lasted several hours. In the animal, on the contrary, death occurred rapidly, and the lung neither presents the same disorders, nor the blood the same degree of fluidity. Here, if I mistake not, are clear and precise applications of the general results to which our researches have led. I had never before had an opportunity of observing, in so direct a manner, the action of carbonic acid on the coagulability of the blood. We now know that that gas liquefies the blood, not only the portion of it circulating through the lung, but its entire mass. We may legitimately go further than this; the phenomenon of exhibition will account for a number of lesions in the other organs that are, beyond all question, primarily produced by the liquefaction of the blood. In my opinion, various morbid changes pointed out by pathologists as occurring in analogous cases, such as dark colour of the intestines, erosions of the mucous membranes, effusions, sanguinolent diarrhœa, &c., are in ultimate analysis, traceable to the same cause. I would, therefore, strongly recommend you to turn your serious attention to facts of this character: examine them with severity and minuteness; if they are, as I believe, real, they both simplify the science and may lead to valuable discoveries capable of changing the face of pathology.

We may now change our subject, and turn to the consideration of other phenomena that have been but little studied, though they are quite as important as those with which we have hitherto been engaged. I allude to the different modifications induced in the blood by variations of temperature. These may be of such a kind as to cause a complete stoppage of the circulation, though that fluid have lost none of its chemical characters or normal properties. This fact has alone been established by experiment. Still, in respect of its connection with the generation of organic changes, medical men have paid it but little attention; they are so absorbed with irritation and inflammation that they think of nothing else, and yet the temperature of the blood deserves to be seriously examined in this point of view. A recapitulation of what is known on the subject cannot be otherwise than useful. Hales, an exceedingly clever natural philosopher, and a man of genius besides, made a great number of experiments on this subject. Their object was to prove that the movement of liquids through tubes was rapid in the ratio of their temperature: the greater their heat, the more swift their circulation. Here was one of his ingenious experiments: he fixed an injecting syringe to the mesenteric artery of an animal, after having first secured the vein of the same name in such a manner that he was enabled to collect and measure the exact quantity of liquid brought back by it. The anastomotic branches in the neighbourhood were also carefully tied. Matters having been thus arranged, Hales states that he ascertained the following facts:—First, cold water pushed into the artery returned by the vein in a given time, but I cannot at this moment recollect the exact figures. Secondly, the same quantity of liquid, of a medium temperature, passed 18 times quicker under the influence of the same pressure. Thirdly, when hot water was employed, the velocity of its passage was found to be 32 times greater than that of the lukewarm fluid. Hales has not mentioned the exact temperature of the liquids with which he experimented. If we suppose that there was a difference of 60 deg. Centigr. between the liquids he called cold and hot, we should find that the water flowed in the latter state with 576 times greater velocity than in the former. Every one is aware that during very hot weather the face is more highly coloured than at other times, and the movements of the heart more rapid. A greater quantity of blood is constantly passing through the vessels, and the cutaneous and pulmonary exhalations are very sensibly increased in quantity; hence the craving we feel for aqueous drinks, in order to restore to the blood the water it is constantly losing. But it is on the capillary circulation, more especially, that the temperature of the blood exercises a very manifest influence. A voluminous artery is more easily traversed by a column of blood than a minute branch. In proportion as the diameter of the vessel decreases the obstacles increase. It is easy to estimate, comparatively, with glass tubes, the mechanical difficulties originating simply in the narrowness of the vascular canals to be traversed; but we cannot with these form any accurate notion of the influence of temperature; and this because the variation of heat acts not only on the blood in the capillary vessels, but dilates the membranous walls of the latter, enlarges their cavity and modifies the hydrodynamic phenomena. Pulsations are felt where they did not exist before; several globules now pass a-breast in the tubes, which scarcely allowed of the passage of one; in a word, a new state of things discloses itself for the investigation of the physiologist. The condition of persons labouring under an organic affection of the heart furnishes a good illustration of this. Cold, damp weather is sure to bring on fits of suffocation in such patients, as well as infiltration and œdema of the lower extremities. Let the atmospheric temperature undergo a trifling rise, and immediately the circulation becomes freer. The symptoms just named then disappear until the return of cold weather. The sufferers themselves are so perfectly conscious of all this that, when you prescribe for them, they tell you a little sunshine would do them infinitely more good than all your drugs. They are right.

When, again, the practitioner wishes to increase the activity of the circulation in an individual and facilitate the passage of the blood through the whole vascular system, he orders a warm bath: its effects are soon felt through the entire frame. The respiration becomes accelerated, because as a greater quantity of blood arrives in the lungs within a given time, a greater quantity of oxygen is required; the tissues swell and the external surface grows red. Again, if the object be to diminish the activity of, for example, the cerebral circulation, and to increase that of distant parts, you make use of pediluvia. The phenomena ensuing are precisely the same as in the preceding case, except that they are simply local. Instead of acting on the entire vascular system, you limit the action of the elevated temperature to a few of its tubes. This, too, is an exclusively physical result. Let us take an example of the opposite kind. An individual puts his hands and feet in snow; the fingers and toes immediately become white, in consequence of the reflux of blood to the central parts of the limbs. The capillaries of the periphery of the body, when submitted to the action of sudden cold, become unfit for carrying on the circulation. Part of the fluid they contained passes into

the veins; and as they no longer admit that which the arteries carry to them, they are really for a moment empty. M. Poiseuille has given the details of several experiments referring to this subject, in his essay on the capillary circulation. He has clearly shown that any notable fall of temperature in those exquisitely minute tubes arrests the course of the blood in them: the globules stagnate and remain immoveable; but when acted on by heat in this state, their oscillation recommences; they assemble in the centre of the vessels, glide over each other, and the circulation is once again in activity. The blood in the living vessels is, in truth, almost as directly influenced by the temperature of the atmosphere as the mercury in the barometer. But how, we may naturally ask, are we enabled to outlive intense frost? Why does not our blood cease to flow, in the same manner as the water of rivers? Because we are provided with an apparatus destined to keep that fluid in a state of constant and equable warmth. Respiration, independantly of its other uses, raises the temperature of the blood by the chemical combination of oxygen with the molecules of that fluid, and by increasing the velocity of its course prevents its solidification. Although these facts are generally known, you will easily conceive that the science would gain materially by our having positive information on the exact amount of influence temperature exercises on the circulation. When we say that it renders it slower or more rapid, there is not enough of precision in our language. We should be able to tell how much slower or more rapid the movement of the blood becomes. With the view of attaining some such accuracy as this, I have planned some experiments which I propose to perform to-day. First, however, I must give you an account of one I performed yesterday. I was anxious to determine with M. Poiseuille what degree of pressure the vessels support, according as the temperature of the tissues to which they are distributed is low or high. We employed the hæmodynamometer, substituting a solution of the subcarbonate of soda for the mercury, in order that the variations in the level of the liquid might be well marked. The specific weight of carbonate of soda being to that of mercury as 1:10, the slightest oscillation of the column was apparent. As the amount of pressure in the veins is much less than in the arteries, it is frequently advantageous, when experimenting on the former sort of vessel, to use this modification of the instrument. We proceeded in the following manner:—The tube was introduced into the femoral vein, with its point towards the capillaries; the paw and the rest of the limb had been immersed previously for a quarter of an hour in a frigorific mixture, composed of two parts of ice and one of common salt. We turned the cock of the instrument, and in 55 seconds the carbonate of soda rose from 0 to 670 millimetres. We then removed the apparatus, and replaced the frigorific mixture with compresses saturated with water at 50 deg. (Réaum.). At the end of half an hour we reintroduced the tube as before into the vein. On opening the cock we found that the subcarbonate took only 38 seconds to rise from 0 to the 670th millimetre of the scale.

The movement of the blood was, therefore, evidently rendered slower by the cold, quickened by the rise of temperature. We should have had still more strongly marked results, had we applied the instrument to a superficial vein, or prolonged the action of ice or cold water. We made the experiment on a deep-seated vein, and acted with our artificial heat and cold on the peripheric circulation only. We will now vary our process. Here is an animal whose limb has been surrounded with a mixture of ice and salt for the last three-quarters of an hour. There must be already a very notable diminution in the progressive force of the blood; but of this we will assure ourselves by the hæmodynamometer charged with carbonate of soda. I now introduce the tube of the instrument into the vena saphena interna, with its point towards the capillary system: the column of saline solution stands at 0. I now turn the cock. You observe that the column rises, but so slowly that any motion in it is with difficulty discernible. The force now moving the blood is evidently much inferior to that in action under ordinary circumstances; not that the energy of the contractions of the left ventricle has diminished, but the mechanical obstacles are more numerous, and in consequence the liquid does not pass so freely from the capillary arteries to the vein. The experiment has now lasted eight minutes, and the upper surface of the column is at the 360th millimetre of the scale; yet the degree of cold is far from intense; a thermometer placed in the wound indicates a temperature of 15+0 (Réaum.).

[The experiment was continued to the 11th minute; the cock being then closed, the carbonate of soda was found to stand at the 620th millimetre.]

We will now perform the converse experiment. I remove the ice, and substitute for it a linseed-meal poultice, warm and thick, just such as would, in hospital phraseology, be called emollient.

[The animal was now left to itself, in order to give the poultice time to act.]

I now replace the tube in the saphena interna; we shall see what time the column of fluid will take to reach the height of 620 millimetres; you remember that in the last instance it took eleven minutes to reach that point. I turn the cock, and you observe that the column rises much more ra-

pidly than in the former experiment. A minute has scarcely elapsed, and the scale already marks 150 millimetres. The pressure is, therefore, sensibly increased. The carbonate of soda continues to ascend in the tube, but its movement apparently becomes slower. Three minutes nearly have passed, and yet it has only reckoned 300 millemetres. I know not to what to ascribe its stoppage, but it certainly stands still at that point. The heart continues to beat, the respiration to go forward. Possibly a clot has formed in the instrument; of this we will assure ourselves. I withdraw the instrument, and turn the cock, yet you see that none of the saline solution escapes. I blow into the upper orifice of the tube, and, as I suspected, would be the case, I have driven out a clot; the liquid now flows out freely; so that there was a mechanical obstacle to the ascent of the column of liquid. The experiment, therefore, though not terminated, is perfectly favourable to our doctrines; for, as far as it went, it confirmed the theory founded on physical laws, which I laid down.

From these, and similar experiments, it follows, that by raising the temperature of a limb we increase the force with which the blood moves in the veins; this fact did not escape the observation of medical men of all ages, and even of non-professional persons. What follows the instinctive action of rubbing the hands when numbed with cold? Disengagement of caloric, and, in consequence, increased velocity of the flow of the blood through the capillary system. The deepened colour of the tissues shows that the liquid moves in greater abundance, and with more facility. But a number of therapeutical principles rest upon these physical data. If you wish to determine a flow of blood to the skin, you prescribe *douches* and the vapour-bath. If a limb becomes cold from having had its principal artery tied, you surround it with bags filled with hot sand. I am well aware that these means are, for the most part, empirically employed. The effect obtained, what matters the cause, you will say? Much; it is repugnant to my reason, I had almost said to my pride, thus to accept *consequences* without inquiry, when it depends upon myself to ascertain their *cause*. I seek, as far as possible, to be both the hand that executes, and the intelligence that comprehends.

It was during the awful epidemic of 1832 that I felt most fully the utility of sound physiological knowledge in guiding therapeutics. In individuals seized with the cholera, the diminished energy of the circulation, and the blue and cold condition of the body, were prominent phenomena; to them, therefore, I gave my chief attention. On the arrival of the patient in my wards he was placed in a thoroughly-heated bed, his limbs rubbed with stimulating liniments, and bags of hot sand applied along the body; internally he was given hot drinks. While I endeavoured, by these physical means, to raise the temperature of the blood, I also gave my attention to supporting the contractility of the heart, and for this purpose employed spirituous and aromatic draughts; and what we see in our experiments with the hæmodynamometer, I have many a time had occasion to verify in the human subject, namely, the re-establishment of the circulation, and in fortunate cases the recovery of health, through the artificial restoration of animal heat. On the other hand, cold baths are recommended in cases where the indication is to soothe general excitement, characterised by excessive activity of the circulation. When the forehead burns, and the temporal arteries beat violently, we apply ice, or some frigorific fluid, to the part. When too much blood flows to the brain, we have recourse to the cold affusion; cold, therefore, skilfully managed, and applied opportunely, is an energetic therapeutical agent.

The treatment of fractured limbs, by constant irrigation with cold water, furnishes another example of this mode of action. The prolonged continuance of a low temperature diminishes the vascular pressure, prevents the blood from rushing in the natural quantity towards the seat of the lesion, and, in a word, prevents the phenomena called *inflammatory* from taking place. When we plunge into cold water, during very warm weather, a peculiar sort of constriction is felt at the thorax. This, again, is a purely physical phenomenon. Less blood traverses the capillaries, consequently it accumulates in greater quantities in the larger trunks; these, distended as they are, press on the neighbouring organs; hence the uncomfortable sensation of which the patient is conscious. It disappears when the equilibrium of temperature is restored throughout the sanguineous system; and so the sudden immersion of the whole body renders its effects much less sensible. Again, to take another instance, what do you suppose occurs, physiologically speaking, when an ice, or a draught of very cold water, is swallowed? We have not investigated with the instrument the effects of the introduction of a cold liquid into the veins; but I do not believe that I shall widely err in saying, that the pressure supported by the vessels must diminish. The refreshing sensation of comfort felt, after drinking iced water, must depend on its rendering the circulation slower; but as all ingested fluids remain in the stomach some moments, before they are imbibed by the coats of the veins, their temperature must have risen by the time they enter the circulation. In order, therefore, to ascertain with accuracy the effects of directly cooling the blood, we will inject cold water into the

veins of the animal. The instrument is, as you see, applied to the right carotid of a middle-sized dog. The jugular vein is laid bare, and a tube secured in its cavity covers the extremity of the injecting syringe. The temperature of the water we are about to inject is 3+0 (Réaum.). I almost fear the introduction of such very cold fluid may at once determine serious functional disorders. The syringe is capable of holding 100 cubic centimetres of liquid. The mercury now oscillates between

70—75; 65—70 millimetres.*

Let us see if the pressure will be diminished or increased in the artery; I presume it will be lessened, but the experiment has never been tried.

1st injection : 70—85; 68—75 mill.
2nd injection : 60—80; 75—85
3rd injection : 65—80; 70—85
4th injection : 65—85; 70—80

It would appear, Gentlemen, that instead of diminishing, the pressure is slightly augmented. I certainly expected quite a different result. I continue the operation.

5th injection : 60—85; 65—85 mill.
6th injection : 70—80; 65—115

The last ascent of the column was caused by a violent effort the animal made at that instant; but it is clear that the pressure is not weakened by the action of cold water; the mercury mounted, on the contrary, a few millimetres after its introduction.

I should be curious to examine the temperature of the blood. We will remove the tube, and collect a little in a vessel. Here is a small quantity, and it marks, as you perceive, 31° Centigr.; hence its temperature is 7 or 8 degrees lower than the normal standard.

7th injection : 75—85; 70—85 mill.
8th injection : 65—80; 65—85
9th injection : 65—85; 70—80
10th injection : 60—75; 65—85

The mercury remains at the same level without any notable variation. My surprise at not seeing the column fall is doubly great, inasmuch as, theoretically, I should have admitted two causes of diminution in the vascular pressure—the temperature of the liquid and its debilitating action. You perceive how the animal is swollen by the accumulation of the successive injections. The coats of the arteries are extremely tense; there is a sort of aqueous plethora.

[M. Magendie now injected fresh quantities of the water; but the mercury still oscillated between 65 and 85 millimetres.]

I have just injected for the eighteenth time, and yet the pressure is not sensibly modified. Nearly two litres† of cold water have passed into the circulation, which is more than enough to produce the effects I contemplated. The result of the experiment is, I freely confess, exactly the reverse of what I had expected. The hemodynamometer has once again corrected my theories; it were to be wished, indeed, that every clinical or physiological professor had an instrument of the kind by his side, to ascertain the real solidity of his conjectures. Unfortunately, there is no such instrument; but if there were, some time would, no doubt, elapse before it came into general use; few people would like to have so indiscreet a contradictor constantly beside them.

I will now beg to call your attention to the influence of the blood on the development of certain physiological phenomena, that have excited considerable interest among clinical observers, since the time of Lænnec. No one had, however, traced them to their true source; for the generality of medical men, unacquainted, as they are, with physics and chemistry, pay no regard to the community of character existing between the normal or anormal phenomena of our organisation, and those of physico-chemical science. Yet, if it be desirable that medicine should cease to be a mere trade, that it should take the foremost place among the departments of human knowledge, their habitual indifference must be got rid of, and they must learn to call to their aid every description of inquiry capable of widening the field of their information, and of smoothing the path to ultimate truth.

Well, let us suppose a chlorotic female admitted into an hospital. On examination we find that the skin is soft and flabby; that all the tissues present a peculiar kind of whiteness, and that the patient feels constantly chilly. These phenomena prove the existence of an alteration in the blood, as well as that the capillary circulation is imperfectly performed. In order to remedy these organic disorders, the practitioner bleeds the patient, and gives preparations of iron, which, according to the current belief, possess the property of recomposing the blood. But, farther, a particular sound is discovered on ausculting the heart and principal arteries. Now, this sound has been carefully described, and called *bruit de soufflet*, *bruit de diable*, &c., but no attempt has been made to find out how it is produced. I have lately proved, Gentlemen, that this sound depends on the nature of the blood; my experiments place the point beyond all doubt. Thus, when we examine into the audible effects of the sudden passage of water through inorganic tubes, we find that only a very slight rubbing sound (*bruit de frottement*) is produced; but if we add to the fluid either albumen, starch, or common salt, a sound is then emitted pretty closely resembling that heard in the arteries of chlorotic subjects; and what strengthens

* The equivalent of the millimetre in English measure is 0.039370 inch.
† The French litre is equal to 1.760 pints.

my conviction of the correctness of this purely physical explanation is, that when the chlorotic state goes off the sounds disappear also. From this I necessarily conclude, that they depend on the composition of the blood, and not on any particular condition of vitality and vascularity, which has, in reality, no connection with them. I intend to repeat my experiments relative to these phenomena, for their importance cannot be doubted. Professor Bouillaud has already made an application of this idea by seeking, in various ways, to estimate the relation existing between the state of the blood and the sounds of which we have been speaking. Among other attempts of the kind, he has measured the viscidity of the blood with the areometer, and has, I believe, ascertained that when that quality is developed beyond a certain degree, those sounds cease to be produced.

The blood is an extremely complex liquid, as may easily be learned, by casting an eye over the numerous treatises that have been written respecting it. As regards the chemical part of the question, a good deal has been done; but if we turn to its physiology and pathology, we find that the very foundation has yet to be laid; but even the few scattered facts heretofore noted, show the importance of its most trifling alterations in the production of various maladies. We are daily consulted by patients who complain, some that they have a "disease in their blood," others that their blood is too thick. Although there is neither science nor merit in these old woman's notions of our patients, still experience proves that there is something of truth at the bottom of them. It would almost appear, indeed, that medical men persist, from mere pettishness, to deny the existence and neglect the study of causes, which the most ignorant persons have divined, simply because they are uninfluenced by the prejudices and the disputes of the schools.

One of the obstacles which interferes most materially with the direct and thorough study of the blood, is the difficulty of examining it in its proper vessels. It is a very different thing to study it with the microscope, while yet within those tubes, or after it is removed from them. No matter what expedients you employ, during the short transit from the vascular tubes to your receiver, the component elements of the fluid are found to affect a new arrangement. The simplest proof of this, that when newly-drawn blood is examined, it is sometimes found wholly coagulated; at other times partly liquid; the clot may even float in the midst of serum: in a word, it presents itself under a variety of aspects; besides, it is impossible to look upon it as a perfectly identical fluid in the same individual, even during a few consecutive hours. It is unceasingly destroyed by excretion and exhalation; and as continually it is renewed by the chyle, by drinks, &c. So that, examining it in the midst of these continual movements of composition and decomposition, it is almost impossible to obtain it in a fit state for comparative study at different periods. The influence of drinks on the blood, especially of those that contain an excess of carbonic acid, is almost instantaneous. Scarcely have we swallowed a glass of Champagne when the blood is modified by venous absorption. In order to establish the reality of this modification I propose getting some one to drink a bottle of Champagne voluntarily, and a few moments after practising venesection, to the extent of one or two ounces. Beer produces very nearly the same effect. Scarcely has one swallowed a glass or two of that liquor, when the urine becomes increased in quantity to such a degree as to require immediate expulsion. These considerations are enough to show that the analysis of the blood is attended with difficulties of a peculiar kind, such as are not met with in the analytic examination of other liquids, such as mineral waters, for instance.

In consequence of this double movement of composition and decomposition in the blood, it becomes necessary to admit in it two principal series of elements. First, those really forming it, which we will call *constituent*; next, those that simply pass through it, which we may term *transitory*. The same distinction holds with regard to our food; some part of it remains as a formative element of the frame, the rest disappears by exhalation, especially that from the lungs. The blood, while circulating, is liquid; but what shows, beyond dispute, that it must not, for a moment, lose the property of coagulating is, that its solidification is necessary for the production and formation of our organs. When, therefore, it becomes unhealthyly fluid, it is rendered unfit for nutrition.

I placed before you, a few days past, the list of substances detected in the blood by modern chemistry. You must have been struck with the variety of those materials,—gaseous, fluid, and solid. One of the most remarkable among them is the phosphuretted fatty matter, analogous to that of the nervous substance, and, like it, causing instant death, when re-introduced into the blood by injection. M. Pinel Grandchamp states, that the injection of half-an-ounce of this substance into the veins of a furious buffalo, proved almost instantaneously fatal. Now death, in this instance, is a purely mechanical result; this I demonstrated clearly last year. The nervous pulp acts solely by obstructing the minute capillary tubes; it is otherwise inoffensive.

Among the more novel substances detected of late in healthy blood are cholesterine, the well-known material of biliary calculi;

seroline, a yellow and a blue-colouring matter. But there is one assertion of M. Lecanu which I am disposed to combat; that gentleman believes, and MM. Prevost and Dumas formerly supported the same opinion, that the fibrine forms part of the globules. It would be impossible to study the blood with any success in its physiological and pathological relations, if the fibrine were looked on as forming part of the globules. If we take the blood of an animal, and let it remained undisturbed, it becomes solid; if, on the contrary, we shake it, or whip it, as the butchers do, it will differ from other blood by our having, through the process alluded to, separated from it an elastic, whitish matter, which is nothing else than the fibrine; and if we examine it in this state, we find the globules exactly as they were, unchanged either in volume or in form.

It is not denied that, if the blood be left at rest, the globules will be found, on analysis, united to the fibrine; but, as when their association is mechanically interrupted, we still find the globules just as they were before, I cannot admit, until some new evidence be found in their favour, the ideas of the physiologists to whom I have adverted. There is another question involved in that just mooted, of still more delicate character, namely, What is the condition of the fibrine in the serum? Is it in solution or suspension? The latter appears to me the more probable of the two. I believe that the fibrine, instead of being a sort of precipitate in the blood, exists in that fluid in *the form of minute vascular arborisations*, forming, in a certain sort, the first degree of organisation. But the problem is by no means solved on account of this preference on my part.

ON THE

PREPARATION AND NATURE

OF THE

IODIDE OF ARSENIC;

WITH OBSERVATIONS AND EXPERIMENTS ON ITS MEDICINAL AND POISONOUS PROPERTIES.

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(Read in the Medical Section of the British Association for the Advancement of Science, at Newcastle, August, 1838.)

SINCE the introduction of iodine as a therapeutical agent some of its compounds have been employed in medicine, and the iodides of iron, of mercury, and of potassium, now hold a place among the preparations of the London Pharmacopœia. The iodides of lead, of arsenic, of zinc, and of sulphur, have, also, been medicinally employed; but little is known respecting their physiological action, and still less regarding their poisonous properties. When we reflect, however, upon the energetic influence which some of these iodides exert on the animal economy, it is scarcely necessary to assert, that an investigation into their mode of operating, whether administered as medicinal agents, or as poisons, is calculated to be productive of very important results, in reference to their therapeutical employment. With a conviction of the value of one of them as a medicine of great power, I propose to lay before the Medical Section of the Association my observations and practical experience of the medicinal properties of the iodide of arsenic, and an account of some experiments with it, as a poison, on dogs.*

This iodide may be made by triturating together 75.4 parts of levigated, pure metallic arsenic, and 631.5 of dry iodine, aiding the combination by a moderate degree of heat; but a better method, suggested by M. Plisson ("Ann. de Ch. et Phys., t. xxxix., 266), is to boil together, in a glass flask, 150 parts of levigated, pure arsenic, and 750 parts of dry iodine, in 7680 parts of distilled water, and when the fluid acquires a lemon-yellow colour, to filter, and to evaporate the filtered fluid to dryness, stirring assiduously, and mixing the concreted portions with the fluid, until the whole becomes solid. This preparation may be, afterwards, sublimed in close vessels, without undergoing decomposition.

The iodide of arsenic, unsublimed, is of a pale brick-red colour, inodorous, and impresses a slight metallic taste on the palate; when sublimed, it forms brilliant scales; when thrown into a quantity of water, inadequate to the solution of the whole of the iodide, decomposition ensues; an acidulous hydriodate, according to Plisson, remains in solution, whilst a sub-hydriodate is precipitated. In a large quantity of water the whole of the iodide dissolves, without decomposition, and forms a lemon-yellow solution, which has an acid reaction, and which is gradually decomposed, when long exposed to the air, assuming a deep brown colour, and exhaling the odour of free iodine. When the solution is made with boiling water beautiful scales are deposited as it cools, which Plisson regards as a sub-hydriodate, and the fluid retains in solution the acidulous hydriodate. I am, however, disposed to regard this supposed hydriodate as a mixture of the iodide and arsenious acid.

The solution of the neutral hydriodate is decomposed by nitric, sulphuric, and nitro-hydrochloric acids, consequently by chlorine, hydrosulphuric acid, solutions of ammonia, sulphate of copper, ammonio-nitrate

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* As the preparation is not very generally known I sent to the Association specimens of it, in its unsublimed and its sublimed state, and also a solution of it.

of silver, bichloride of mercury, the nitrates of mercury, the acetate and diacetate of lead, chloride of lead, nitrate of lead, chlorides of gold, of copper, and iodide of potassium. Ammonia changes the colour to a pale violet hue, but no precipitate falls. The solution may be administered medicinally in combination with the infusions or the decoctions of cascarilla, cinchona, cusparia, and solutions of catechu, guaiacum, and opium, no decomposition taking place.

Although this iodine readily sublimes, and forms beautiful crystalline, brilliant scales, yet, if the temperature be raised to 280° Fah., decomposition takes place, and iodine and arsenic are separately sublimed.

The composition of the dry iodide of arsenic is 2 eq. of arsenic + 5 eq. of iodine ($Ar.^2 I.^5$), making the equivalent 706.9.

The physiological action of the iodide of arsenic, on the animal economy, closely resembles that of arsenious acid, but it is modified by the iodine. In minute doses it is rapidly carried into the circulation, and is, probably, decomposed, and the iodine converted into hydriodic acid; the iodine can be detected in the urine and the other secretions, soon after the iodide has been taken, but I have never been able to detect the arsenic in any of the secretions. Its influence, in the first instance, is that of a tonic, the appetite is increased; but after its use has been continued for ten or twelve days, a degree of pain is experienced at the epigastrium, accompanied with thirst, a dry state of the throat, slight fever, and, sometimes, with diarrhœa and tenesmus; the skin, also, becomes dry, and the urinary secretion is augmented in quantity. If the use of the iodide be still prolonged, the nervous system is rendered extremely irritable, and wakefulness supervenes. I have never seen it cause salivation, which occasionally results from the long-continued use of the arsenious acid. M. Biett had employed this iodide as an external application, in the form of ointment, in some inveterate cutaneous affections; but I am not aware that it was ever administered, internally, until the month of June, 1835, when I ventured to prescribe it, in minute doses, in a long-standing case of severe lepra *vulgaris*. The beneficial influence which it displayed in this case, and the rapidity with which the disease was cured by it, induced me to prescribe it in many other diseases; in scarcely any instance has it disappointed my expectations. I consider it nearly as a specific in chronic impetigo, a disease which has resisted almost every other remedy. It operates most powerfully upon the capillary system, setting up a new action there, and to this influence on the cutaneous capillaries we must ascribe the beneficial influence of the iodide as a therapeutical agent. When the action of the medicine has been closely watched, and its administration discontinued as soon as the throat becomes sore, or pain in the epigastrium is experienced, I have never seen its employment followed by emaciation, nor the softening, nor the wasting of glands, nor by irritative fever, such as occur when iodine is administered; on the contrary, the health has improved, the strength has become augmented, and the body has obviously increased in bulk.

As illustrating its therapeutical action I have selected two cases, from a considerable number, in which I have prescribed it, both in private and in hospital practice.

Case 1.—Charlotte Brooks, aged 50, was admitted into the University College Hospital on the 13th of June, 1837. About twelve months before her admission she was attacked with pain in the left breast around the nipple; the breast was hot, red, and indurated. Various medicines were administered, but, nevertheless, the pain continued to increase in severity, and was augmented by pressure; it extended to the right shoulder, and down the side. These symptoms, without much change or intermission, continued for nearly six months, when a number of small, hard tumours formed in the axilla, and spread over the chest, involving both mammæ; the tumours itched and tingled, but they were not attended with pain, except on the mammæ, which gradually diminished in size, and became indurated and fixed.

When this patient was admitted into the hospital both mammæ were very much shrunk and wasted, the adipose matter was completely absorbed, but the nipples still existed; the integuments around them had a reddish-brown tinge, and was much puckered; there was no suppuration, nor any kind of discharge from either breast; the indurated tumours were spread over the chest and the abdomen, the back, the shoulders, and they were crowded in the axilla to such an extent as to impede the motion of the arms; several tumours were also scattered over the neck, the face, and the right arm and leg; they were of all sizes, from the size of a millet-seed to that of a small nut; the skin covering the large tumour was slightly red, but, on the small, it was of the natural colour; the countenance of the woman had little or no cachectic character; the surface was harsh and dry, and she stated that she never perspired; the bowels were always costive; the tongue was slightly furred; the pulse small, sharp, and quick; she had occasional pains in the head, and dimness of sight. One of the tumours was cut out, and examined; it was pale, nearly as firm as cartilage, and somewhat lobulated, characters depending, in a great degree, on the skin being the seat of the disease.

The difficulty of forming a correct diagnosis of this disease may be readily conjectured; the character of the tumours; their

globular form; the grouping together of several lobules in one common capsule; their whitish colour, uniform texture, firm, almost cartilaginous, consistence; and their tendency to soften as they enlarged; their chiefly affecting the mammæ and the axilla; the fixed, indurated state of the mammæ, and the lancinating, intermitting pains which darted through them, more severe at one time than at another, and extending down the arms, were symptoms which led to the idea that they were of a *carcinomatous* nature; and this opinion was supported, also, by the weak and emaciated condition of the patient; on the other hand, the absence of the sallowness of the countenance, which always, more or less, attends cancerous cachexia, the absence of irritative fever, and the otherwise good state of the general health, were unfavourable to the opinion that the disease was carcinomatous; it resembled no cutaneous affections which I had ever seen; it could not be confounded with molluscum *pendulum*, which it approximated more than any other skin disease, especially when the tumours are not pendulous. In this dilemma I resolved to treat it as a carcinomatous affection, and to give the iodide of arsenic a fair trial.

June the 14th, after evacuating the bowels freely with calomel and a senna draught, the following pills were prescribed:—

℞ *Iodide of arsenic*, gr. i;

Extract of conium, 3ss. Make two equal pills; take one every 8 hours.

She was ordered to take three grains of iodide of potassium, and thirty-six minims of liquor potassæ, between each dose of the pills, and a pill at bedtime, consisting of two grains of blue-pill, and three grains of extract of conium. Little alteration was perceived until the 19th, when the tumours became more moveable, and the clusters in the axilla were evidently lessening in size. On the 23rd the mammæ became more moveable, and the skin over them less tight than before, and every day afterwards presented some improvement in the appearance of the tumours. On the 28th, the patient having complained of pains in the stomach and the abdomen, the use of the iodide was discontinued for a few days, and the bowels were ordered to be freely opened; but she resumed its use on the 2nd of July. The same plan of treatment, with little variation, was persisted in until the 28th of August, the tumours gradually lessening in size and disappearing, and the general health of the woman improving; on this day scarcely a single tumour remained, she was, therefore, discharged from the hospital cured.

Several cases of severe impetigo were successfully treated with the iodide of arsenic, in doses of the sixth of a grain, at the same time that small and repeated bleedings were resorted to, and the decoction of sarsaparilla administered; they all recovered rapidly, and the patients have remained well.

The only other case which I shall mention is one which occurred in my private practice; it was one of carcinoma of the mammæ, which was effectually checked by the iodide of arsenic, in conjunction with the iodide of iron; the patient, at the termination of two years, remains in perfect health.

Case 2.—Mrs. M., a lady 46 years of age, applied to me on account of a tumour in both breasts; on one side the tumour was fixed, on the other it was moveable; on both sides there were occasional severe darting pains, the arms had become œdematous, the complexion was sallow and anxious, there was constant fever, and the health was rapidly sinking.

On examining carefully the nature of the tumours, and the general symptoms, I was convinced that the disease was incipient cancer, and my opinion was verified by another physician and a surgeon.

The plan of treatment which was adopted consisted in the application of ten or twelve leeches, at short intervals, with poultices composed of poppy-heads, digitalis, and conium, and the internal administration of the iodide of arsenic, in doses of the eighth of a grain, with extract of conium every eight hours; the extract of conium was, at first, given in doses of three grains, but the dose was gradually augmented, until a drachm was taken in the course of each day. Three grains of iodide of iron, in solution, were taken between each dose of the pills; the patient was strictly confined to a milk diet, and the use of every kind of stimulant was interdicted.

This plan of treatment was pursued for upwards of eleven months, during which time the iodide of arsenic was carried up to doses of the third of a grain, beyond which it always displayed acrid properties, which rendered it necessary to reduce the dose. The tumours gradually became softer, and disappeared, the pains ceased, and the general health improved in a striking manner, until the patient was completely freed from disease.

It might be supposed that, in the last of these cases, much was due to the influence of the conium, and to the mild diet of the patient; and it cannot be denied that conium, with a regulated, mild diet, exerts a most powerful influence in carcinomatous affections. But, in treating other cases, in the same manner, without the iodide of arsenic, the same beneficial result has not been obtained. It appears to me that the iodide, operating upon the capillary system, so greatly improves the general glandular and secreting functions, as not only to check the deposition of the carcinomatous matter; but to enable the system to throw off that which is already deposited. The absorption of the

iodide is demonstrated by the detection of the iodine, in the form either of the iodide undecomposed, or of hydriodic acid, in the urine, a few days after the use of the medicine has been commenced; and, as it cannot be detected sooner than two or three days, we may conclude that it accumulates in the system. In the minute doses in which it should be administered, if its effects be closely watched, it is a safe remedy; but, as it is much more energetic than the arsenious acid, and as the habit does not appear to accommodate itself so readily to its influence, as to that of arsenious acid, the dose must be augmented with great caution; and its employment should be altogether suspended on the first indication of its topical acrimony.

I have found that the same circumstances, which contraindicate the internal employment of arsenious acid, influence the administration of the iodide of arsenic; namely, an irritable or very plethoric condition of the habit, the presence of inflammatory fever, and the hæmorrhagic diathesis. When phthisis or hectic fever is present, iodide of arsenic should not be prescribed. I have not found any disadvantage to attend its internal employment in children; on the contrary, I am, at present, administering it to a girl of twelve years of age, labouring under impetigo, in larger doses than I have yet ventured to prescribe it to adults, without the smallest deleterious effect.

I have already mentioned that M. Biett has employed this iodide, as a topical application, in the form of ointment, in some cutaneous diseases: but, from the result of its application to a wound, in one of the experiments which I am about to detail, the propriety of great caution, in applying it to a denuded surface, will be evident.

Tests of the Iodide in the Secretions and the Blood.

Such are the results of my observations of the *therapeutical employment* of the iodide of arsenic: as it might be overdosed or be given by mistake for the red sulphuret of mercury, it was important to ascertain its mode of operating as a poison; the following experiments upon dogs were instituted for that purpose.

Before detailing these experiments, I may mention the method which I have adopted to test the presence of the iodide, in the secretions and in the blood.

In testing the urine and the bile, it is only necessary to mix with the fluid some cold mucilage of starch, in a *cylindrical* vessel; and then to pour over the mixture *gaseous* chlorine. If any compound of iodine be present, it will instantly become obvious by the formation of a blue ring of iodide of amidine, at the surfaces in contact. The advantage of employing chlorine, in the gaseous state, is the impossibility of over-charging the fluid with the test, a circumstance which often defeats our object, when the chlorine it used in aqueous solution. When a cylindrical vessel is employed, and the gas is poured upon the mixture, the decomposition of the iodide takes place only at the point of contact. By gentle agitation, new surfaces are successively brought into contact; and a most decided evidence of the presence of free iodine is thus obtained.

In testing the saliva, the moisture of the pleura, or of the peritoneum, and the matters evacuated by purging or by vomiting, I have found that the best method is to use slips of bibulous paper, prepared by soaking them in mucilage of starch and then drying them. These slips, moistened with the substance to be tested, and, immediately afterwards, introduced into a bottle of chlorine, indicate, by acquiring a blue colour, the presence of free iodine. This mode of testing is sufficiently delicate for most purposes.

Blood, suspected to contain a soluble iodide, ar any compound of iodine, should be diluted with water and boiled, in order to coagulate the albumen and the fibrine, and to procure a clear colourless fluid by filtration; this must next be concentrated by evaporation, and when it is cold, mixed with starch, and tested, as already described, with gaseous chlorine. This, however, only indicates the presence of the iodine; in order to determine that it is iodide of arsenic, acidulate the filtered and concentrated fluid, and pass through it a stream of sulphuretted hydrogen, which converts the iodine into hydriodic acid, and precipitates the arsenic as a sulphuret, which may be reduced in the usual manner, whilst the supernatant fluid is tested with starch and chlorine.

The iodide of arsenic itself, either in the solid state, or in solution, may be tested with the sulphuretted hydrogen and the chlorine; and a demonstration readily obtained of the iodide, by the detection of both its components.

In its physical character, the iodide of arsenic, in the sublimed state, closely resembles the sublimed iodide of antimony. In the unsublimed state, the difference is at once perceptible. The one is, however, readily distinguished from the other, even in the sublimed state, by the solubility of the iodide of arsenic, whilst the iodide of antimony is instantly decomposed, when it is thrown into water, and is converted into *hydriodic acid*, which is dissolved in the water, and *protoxide of antimony*, which remains as a precipitate.

EXPERIMENTS ON DOGS.

In the following experiments, the œsophagus of the dogs, to whom the poison was administered, was tied. When the iodide was given in substance, it was made into a bolus with linseed meal, and administered

by the mouth before tying the œsophagus; when it was administered in solution, the fluid was injected into the stomach, through an opening made in the œsophagus, and the ligature applied below the opening. The phenomena were noted down as they occurred, and the exact time of their appearance, from the introduction of the poison, minuted by a watch. The anatomical examination of the body was made as soon after death as it could be effected.

Experiment 1st.—One drachm of iodide of arsenic, dissolved in four ounces of distilled water, was injected through an opening, in the œsophagus into the stomach of an apparently healthy dog, and the œsophagus tied.

In five minutes, the animal became sick; but, in the efforts to vomit, the œsophagus having burst, near the cardia, the observations upon this experiment were discontinued. It is probable that the bursting arose from some previously diseased condition of the organ.

Experiment 2nd.—One drachm of iodide of arsenic, dissolved in four ounces of distilled water, was injected, as in the former experiment, into the stomach of a strong, healthy dog; and immediately afterwards, the œsophagus was tied. Symptoms of sickness displayed themselves, four minutes after the administration of the poison: these were quickly followed by most severe and almost unremitting efforts to vomit. The pain at the stomach was evidently great, as the animal frequently pressed the abdomen, on the floor, then hastily rose, and rapidly passed from one place to another, as if seeking relief. The augmented secretion of saliva, which was extremely viscid, and difficult to be removed from the mouth, greatly annoyed the animal.

Twelve minutes after the administration of the poison, he had a fluid stool, consisting chiefly of mucus; this was followed by another, which seemed to afford some momentary relief. With the second, a tape worm, ten or twelve yards in length, was ejected; both stools, as might be suspected contained the iodide. The heart was beating very irregularly; indeed, so much so, that no indication of the character of its beat could be obtained by means of the stethoscope. The pulse was extremely rapid and sharp; and spasmodic twitchings affected both the fore and the hind extremities. The efforts at vomiting, which had ceased for a few minutes, recommenced with increased severity, accompanied, in the intervals, with general tremor, and an evident failing of muscular power in the limbs.

In twenty minutes, the animal fell on his side; and, as he lay, the *left* fore leg and the *right* hind leg, were affected with continual twitchings, whilst the two other limbs remained at rest. In thirty-seven minutes, the other legs were affected in the same manner: the muscles of the lower jaw were also convulsed. In five minutes more, the convulsive twitchings became general; and the breathing was much embarrassed. The respirations were strong, deep, convulsive, and only twenty in the minute.

In forty minutes, the moanings of the animal indicated increased suffering, the intercostal muscles were spasmodically affected, whilst the general twitchings continued.

At the end of the hour, *emprosthotonos* supervened; the respirations were deeper and at longer intervals than before: the twitchings remained unabated. The opposite legs, which had been first affected by the twitchings, now became paralysed, whilst the others continued in spasmodic action. The sensibility of the dog was unimpaired; for, on pinching the ear, he uttered a cry, and the eyes closed when the fingers of any person were approached to them.

Ten minutes after the hour, the violence of his pain forced the poor animal upon his legs, but he instantly fell, and never rose again. In two minutes more, the right hind leg became paralysed, but the left fore leg continued under the influence of the twitchings: he was still, however, sensitive. At the end of an hour and forty-five minutes, all sensibility was lost, except at the muzzle. At the termination of two hours, he was still alive, but totally insensible. He died, two hours and a half after the administration of the poison.

The body was opened in less than an hour after death. The lining membrane of the œsophagus and of the stomach displayed a deep red colour, the result of strong inflammatory action; in the stomach, the mucous membrane was softened throughout, and thinned, almost to the peritoneal coat, in the cardiac portion. The iodide was readily detected in the softened tissue. This thinning was evidently not the result of ulceration; but seemed to have been produced by a change of the tissue, and its conversion into a gelatinised substance.

The veins of the peritoneal covering of the stomach were excessively turgid; but this coat was not *decidedly* inflamed. The duodenum participated in the inflammatory state of the stomach; and, in a diminished degree, the redness extended through the whole length of the jejunum and ileum; the rectum, also, was particularly inflamed; and ecchymosed patches were dispersed over it, whilst the ridges of all the rugæ were of a deep red. There were no red spots on the mitral nor on the tricuspid valves, nor on the columnæ carneæ of the ventricles of the heart, such as are frequently observed in cases of poisoning by arsenious acid. Both sides of the heart were turgid with *coagulated* blood; the lungs were collapsed, but they bore no traces of inflammatory action; they did not crepitate. The mucous membrane of the trachea was slightly inflamed.

Neither the liver nor the spleen presented any morbid appearance; the kidneys were natural; the urinary bladder contained about an ounce of urine.

The vessels of the brain, and those of the spinal cord were more than usually turgid; but, neither of these organs presented any traces of inflammatory action.

The presence of the iodide was detected in the contents of the stomach, the saliva, the mucus of the trachea, the bile, the urine, and the blood in both sides of the heart.

Experiment 3rd.—This experiment was made merely with the intention of confirming the results of the former. It was, therefore, conducted in a similar manner, on a dog as nearly as possible of the same size and strength, as the former; the same quantity of the iodide was employed. The results were, in every respect, with the exception of death not occurring so rapidly, the same as in the former experiment.

Experiment 4th.—In this experiment, my object was to ascertain what quantity of the iodide of arsenic would destroy a strong dog, when the œsophagus is not tied.

Four grains of the poison, made into a bolus with linseed meal, were administered by the mouth, to a strong dog, of the mongrel breed. In four minutes he became sick, and vomited a white, frothy matter tinged with the yellow colour of the iodide. The vomiting was repeated at intervals, of a few minutes, five times successively, after which the animal lapped some water, and then lay down and slept. Next morning, he appeared quite well and rather more lively than usual.

Eight grains of the iodide were, now given in the form of a bolus, as yesterday. The animal did not vomit until half an hour had elapsed: and the vomiting was only once repeated. Next morning he appeared to be in perfect health.

Twelve grains of the iodide were then administered, with nearly the same result that followed the administration of the eight grains.

On the next morning, twenty grains were administered, with a similar effect to that of the smaller doses, except that the vomiting succeeded the administration of the poison in two minutes; and was more frequently repeated than on any of the former occasions. Finding that the vomitings rendered the poisoning ineffectual; a scruple was this morning, Monday, (two days after the last twenty grains were given), administered in the form of a bolus, and the œsophagus was tied. The symptoms which followed were nearly the same as those of the second experiment, except that they were less violent; and the animal, having lived the whole of the day, died in the night.

On the following morning, the body was opened and presented the following appearances:—

The stomach was not so red as in the second experiment; but it was more extensively softened, as may be observed in the drawing,* and was also ulcerated in several places. Upon two spots a quantity of coagulable lymph was deposited, and adhered firmly to the coat beneath it. The vessels on the peritoneal covering were much injected. The traces of inflammatory action were not considerable in the duodenum, and they became less and less conspicuous until they disappeared in the small intestines. In the portion of the ileum approaching the caput coli, streaks of red again displayed themselves; these increased in the rectum, on the summits of the rugæ of which were many ecchymosed spots. The lungs, the heart, the liver, the spleen, and the brain, presented the same appearances as in the second experiment.

The presence of the iodide was detected in the blood, the bile, the urine, the saliva, the moisture of the pleura and that of the peritoneal sac. It is scarcely necessary to add that it was abundantly procured from the matters vomited, and passed by stool, before the œsophagus was tied.

Experiment 5th.—Half a drachm of iodide of arsenic, dissolved in four ounces of water, was injected into the abdominal cavity of a bitch.

In ten minutes vomiting supervened, and was repeated three times, in the space of two minutes, when the animal fell on her side; the breathing suddenly stopped, and in half a minute more the action of the heart also ceased; a few seconds before death took place, violent spasmodic twitchings affected the back.

On opening the body, immediately after death, the peritoneum appeared highly inflamed; neither the peristaltic nor the vermicular motion of the intestinal canal was evident. The lungs were greatly congested and red: the right side of the heart was turgid with blood; the left was nearly empty.

During the life of the animal, the iodide could not be detected in the urine, which was passed five minutes after the administration of the poison: but it was detected in milk pressed from the teats of the animal, who was in the second month of pregnancy. After death, it was detected in the blood, the bile, the urine, and in the moisture of the pleura, but I could not detect it in the saliva.

Experiment 6th.— Half a drachm of iodide of arsenic, dissolved in four ounces of distilled water, was injected into the right side of the thorax of a strong dog. In five minutes, the animal fell on his side, and

* A coloured drawing was exhibited to the Medical Section when the paper was read at Newcastle.

displayed violent tetanic convulsions: the breathing was extremely slow, and altogether abdominal. In ten minutes, the action of the heart became still slower than before, and intermittent; in eleven minutes, the fore legs were affected with violent movements, and the whole habit suffered under tetanic spasms; and, in fifteen minutes, the animal was dead.

The body was opened immediately after death. The lungs were collapsed, devoid of crepitation, and of a whitish-brown colour.

The diaphragm could not be excited to action by pinching or irritating the phrenic nerves.

The heart was turgid with blood on both sides: it was completely insensible to stimuli, either mechanical or chemical: but the fibres of the systems exhibited a tremulous movement, which continued for ten minutes. On opening the heart, the tissue and the valves appeared natural.

The pleura of the right side was slightly reddened: that of the left was natural.

The stomach was much thickened and inflamed; and the duodenum also displayed traces of inflammatory action.

The iodide was detected in the blood of the left ventricle of the heart; in the stomach; and in the saliva. It could not be found either in the bile, or in the urine, a circumstance which can only be accounted for by the rapid death of the animal.

Experiment 7th.—Two scruples of iodide of arsenic were inserted into a wound made in the back of a stout dog. The poison caused great pain immediately after its insertion. In fifteen minutes much frothy saliva flowed from the mouth; and when tested, it was found to contain traces of the iodide. In forty-three minutes, twitchings of the skin supervened; and the action of the heart became too rapid to be counted. In seventy-six minutes, the animal became dull, laying the head upon the ground, and was affected with general tremors. In eighty-six minutes the tremors continued and he remained in a stupid condition. In three hours and a half, an opaque, dirty, white discharge exuded from the wound. He died in the night.

The dissection of the body demonstrated that the stomach was natural, but the rectum exhibited traces of inflammatory action. The vessels of the brain were more than usually turgid.

The iodide was detected in the blood, and in the urine.

Experiment 8th.—Five grains of iodide of arsenic, dissolved in one ounce of distilled water, were injected into the jugular vein of a dog; no effect whatever was produced upon the heart. The same quantity, injected a second time, was equally innocuous; but on injecting fifteen grains, dissolved in two ounces of water, the action of the heart stopped in twenty seconds. On opening the body, immediately afterwards, the irritability of the organ was found to be completely destroyed. No traces of inflammatory action were observed on any of the organs; not even on the lining membrane of the heart.

From the foregoing observations and experiments, we may venture to draw the following conclusions:—

1. That the iodide of arsenic, administered in non-poisonous doses, is carried into the circulation, and excites generally the capillary system, augmenting the energy of the secreting function, and operating as a powerful and beneficial alterative.

2. That it enters all the secretions; consequently, when therapeutically employed, it may be administered to infants at the breast through the milk of the mother.

3. That in moderate doses, not too long continued, it improves both the digestive and the assimilating functions; invigorates the muscular power, and augments the bulk of the body.

4. That the same salutary effects are obtained from its employment, as a therapeutical agent, whether it be administered in the solid form or in solution.

5. That when its internal use is long continued, it accumulates in the system, and displays symptoms of poisoning, namely, pain at the epigastrium and in the lower bowels, tremors, and general febrile excitement.

6. That, in large doses, it is a most virulent poison, inflaming the tissues to which it is applied, and softening and gelatinising the mucous membrane of the stomach, and, occasionally, causing ulceration.

7. That, when administered in large doses, or applied to mucous or serous surfaces, or to an ulcer or a wound, it is absorbed, and exerts a deleterious influence on the nervous centres and the heart.

8. From its effects when its solution is injected into the veins, and from both sides of the heart being found turgid with coagulated blood, when death ensues from large doses introduced into the stomach, we may venture to conclude that, independant of its topical action, it kills chiefly by its powers of destroying the irritability of the heart.

THE

CAUSES, NATURE, AND PREVENTION OF SMALL-POX.

By Francis Eagle, *Esq., Surgeon, London.*

"Upon the study of physical philosophy depends the further advancement of the science of medicine," observes Dr. Hilary, in his philosophical work on small-pox, a truism which should form the frontispiece of every school of medicine in the world.

Fully convinced of its truth, I betook myself, some four years since, to examine

some of the generally prevailing theories of disease. Having already investigated the nature and origin of both syphilis and hydrophobia, I now approach the subject of small-pox, in which the same obscurity prevails. All is darkness, save here and there a momentary flash of light, whose interval is centuries, and which, even then, does but serve to make our darkness still more visible. Thus, Isaac, the Jew, the Arabians, and Avicenna, assigned as its cause the impurity of the woman's blood, stagnant in the woman with child, and with which the birth was nourished in the woman.* Upon this theory Dr. Gideon Harvey, in 1696, published an admirable commentary, and, apparently, to his satisfaction refutes it altogether. He also observes, that small-pox in children is much less dangerous than in those who are grown up.—(Page 92.)

Dr. Black, 1781, informs us that small-pox is believed, in India, to have existed from time immemorial, and that in the Gentoo code of laws (which are said by the Brahmin to be 3370 years old) there is a form of divine worship and prayer, instituted to be offered up to the *Goddess of Spots*, a supposed female divinity, and that these religious practices are faithfully observed during small-pox and measles, &c. It further appears, that delegates from different colleges of Brahmins are appointed to travel annually, every spring, through the provinces, to inoculate. On which I shall only remark, that it is curious the object of worship is *a goddess*. Dr. Mead believed it to have originated in the hot sands of Ethiopia. How, when or wherefore does not appear. Sydenham advises that we do not waste our time by looking after the causes of this disease; and it is the same author, I think, who uses the curious expression,—"That he who applies himself to find out the cause of small-pox, must be equally mad with those who sought after the philosopher's stone."

Dr. Jenner's opinion that the disease originated at some remote period from the cow, or some other animal, and also the tradition of its origin from the camel, are equally gratuitous assumptions, mere conjecture, and nothing more; nay, the very discovery of cow-pox and its properties affords the strongest possible argument against the derivation of the small-pox from the cow, since one is a local, the other a constitutional affection. Of the origin of this and other specific poisons, Dr. Hamilton logically remarks,—"The same causes which produced it at first will form it a second time, and that which gave it birth a second time may equally produce it a third, a fourth, a fifth, and times innumerable."—(Vol. ii. p. 164, on Hydrophobia.)

* See Isaac the Jew. Liber Octavus, Theorice.

Children are much less liable to Small-pox than the Adult.

Rhazes, Dr. Woodward, Dr. Gideon Harvey, Dr. James, Dr. Douglas, and others, especially the physicians and practitioners of the old school, make this observation, and this is one of the principal reasons why inoculated small-pox is less fatal than natural small-pox. Hence Dr. Gregory's observation, "the best age for inoculation is childhood."

Females are more liable to the Disease than Males.

Van Swieten, in his Treatise on the "Diseases of Armies," does not even mention variola. Sir James Macgregor also says, of 10,000 troops in two years,—"The cases of small-pox were extremely few. As in India, we remarked that in Egypt the contagion did not spread so widely as in Europe; though, at different periods, it broke out in the hospital of the 10th Regt., at Rosetta, in the Department of the Commissary of Cattle, when encamped near the Lake Mareotis, and in the Garrison of Ghiza, yet in none of these instances did the disease spread." Chisholm, Mosely, both, on "The Diseases of Tropical Climates," make no mention of small-pox, or indeed the other exanthemata.

The statistical report of the sickness, &c. of 86,661 troops in the West Indies, extending over a period of 20 years, and containing 164,935 sick admissions, shows that only 13 admissions of this number were for eruptive fevers. Dr. Ferrier, who published an account of the diseases of the French army previous to the year 1796, and Baron Larrey, who also wrote the history of the French army in Egypt, are both silent on the subject of variola, which it is, therefore, fair to infer, did not occur, as other exanthemata are mentioned. Dr. Percival also observes, that of the number of deaths at Warrington, 1773, two-thirds were females. How far the army at home, and the navy abroad, and the history of nunneries, may elucidate this view, I am unprepared to state; but from a casual glance into one or two French works, I am disposed to believe that they all enjoy considerable immunity from this disease. I am, however, unwilling, at this moment, to press the bearing of these facts, especially as I have no doubt as to the peculiar physical condition which directly produces small-pox; and the following evidence is, I hope, sufficient for all practical purposes.

"The deviation of man from a state in which he was originally placed by Nature, seems to have been to him a prolific source of diseases."

Strange that, whilst Dr. Jenner uttered the above, it did not also strike him that the deviation of the cow from a state of nature, would also be followed by similar effects,

one of which was cow-pox. In relating the different cases of original cow-pox the reader will find that Dr. Jenner almost invariably states that the disease broke out in the spring. Mr. Aikin, who published a very excellent condensed account of the causes and history of cow-pox, thus describes the disease:—"The cow, though in general a healthy animal, is subject to some particular diseases, many of which she probably owes to her domestication and intimate connection with man. Some of them have their seat in the udder, especially whilst it is performing the important office of the secretion of milk, and these it is now become of peculiar consequence to know and to discriminate with accuracy." From the observations of those who are conversant with this animal, it appears that there are several causes which may produce sores upon the udder and teats, especially such as excite any irritation upon these organs during the season when the secretion of milk goes on with the greatest vigour. Another and a more serious disorder in these parts is sometimes produced by suffering the cow, while in full milking, to remain for a day or two unmilked, in order to distend the udder when naturally small, &c. &c. The matter discharged from these ulcers will communicate a similar pustular disorder to the hands of milkers when the skin is broken in any part, and often affects them with foul and extensive ulcers that sometimes occasion pustules on the arms and shoulders, and prove tedious and difficult of cure. A suppression of milk in puerperal women often affords a parallel instance of the formation of abscess, though in them the progress and form of the local disease is somewhat different. But the genuine cow-pox is a distinct disease from those abovementioned; it generally makes its appearance in the spring, and shows itself in irregular pustules on the teats or nipples of the udder. Upon inquiry of milkers, and also of the owners of cows in and about London, when the cow-pox occurs, their answer is "grass time;" and I have myself little doubt but that it occurs in Great Britain fifty or a hundred times per annum. By the observation of effects it is that we learn to trace out causes. The first effect worthy of examination is, that vaccinia or the cow-pox is an effect most commonly observed in the dairy counties (*Jenner, Woodville, &c.*), and in the cow or breeding animal; it is not noted in the bull or ox, or calf, or, so far as I can learn, even in the heifer; and notwithstanding that these four latter descriptions of cattle are never handled after the manner of the cow, the fact is still important that the disease appears confined to the cow. Now what is the peculiar condition of the cow in dairy counties, as distinct from other counties? Essex, for example, is famed, all England over, for the superiority of its calves; dairy counties for their milk and butter, and cheese. Generally speaking, the physical condition and circumstances peculiar to the cow in dairy counties consist in this,—that her young is separated from her in order for the different appropriation of her milk; this, then, is the first interference with the "law of Nature," which produces irritation of the uterus; this, again, leads to sympathetic affection of the udder and "milk fever," in many instances followed by an eruption of pustules called cow-pox; this condition of the cow is not very different from that of the human female when placed under similar circumstances, nor to that of the mare and swine, both which have occasionally, according to medical records, originated this disease; but they are rarely placed under similar circumstances, being seldom separated from their young at so recent a date, unless by their death; therefore "the circumstances not being the same neither are the effects the same." Again, if the symptoms of small-pox are carefully followed out, there appears strong positive ground for the belief that small-pox is really in its nature perfectly identical with cow-pox, for—

1st. They are identical in the period of their occurrence, namely, spring and autumn—(Jenner, Estlin, Lancet, April, 1836, Rhazes).

2ndly. They are identical in locality and nature, just so far as is possible from the difference of anatomical and physical formation.

3rdly. They are identical in effect, and "identity of effects" alone warrants the deduction of "identity of cause."

4thly. That there *is* "identity of cause," it is assumed, because all diseases manifest a decided preference of action for that species of animals in which they originate; and, further, this "law" holds equally good of the same animal at different periods of its existence.

Examples of the former,—hydrophobia in the dog, syphilis in man, &c. &c.

Examples of the latter,—porrigo, pertussis, &c., in childhood; syphilis, the plague, in the adult; puerperal fever and variola in women.

Here, too, will be found the causes of the comparative failure of vaccination gradually from the period of its first introduction; not that the vaccine matter has undergone any absolute change, but that the virus of the small-pox, *annually reproduced*, is, by degrees, modified by the causes which produce it, like every other epidemic, such as fever, scarlatina, rubeola, plague; for all diseases, whether isolated or epidemic, endemic or foreign, contagious or non contagious, do, at a greater or less interval, constantly cease with the causes which produce them; and although the same in their specific nature, still they have certain peculiarities in their character and type, so that

scarcely a single epidemic disease observes constantly the character of its predecessor, whatever be its nature, whether fevers, the exanthemata, or any other diseases.* Thus the causes of intermittent fever, in this country, are fast subsiding; the fever itself subsides in a direct ratio; but the causes of intermittent fever continue in the West Indies, and the tropical countries, so also do the effects continue. The tropical zones have their diseases; the temperate theirs; the frigid, also, theirs. Again, divided into kingdoms,—kingdoms have their peculiar diseases; counties, again, theirs; villages their peculiar diseases; descending lower still, families have their peculiar physical condition, loosely called "hereditary;" individuals theirs, also, vaguely called "idiosyncrasy."

Here cow-pox prevails, because its peculiar physical causes prevail; there broncho-cele, because its peculiar physical causes are also present; everywhere more or less continued fever, because wherever mankind inhabits he also eats; and there, also, the physical causes of fever exist. That climate and locality influence its character will not be denied; nay, is an additional proof of the truth of this philosophy. The animal kingdom has its diseases; the vegetable and the mineral, also, theirs. Again, genera have their diseases; species theirs; varieties theirs; individuals theirs; and, lastly, the same individual, at different periods of its existence; they deviate just so much as their physical causes deviate, they approximate, and are identical, just so far as their physical causes approximate; and this, I aver, is true philosophy, the shadow of the substance which shall come hereafter. In order, therefore, to get the full effect of vaccination, the matter should be taken annually from the cow, or just so often as the small-pox is reproduced: the greater identity of cause insuring a greater identity of effects; and it will still further be desirable that the spring be chosen for its communication to the human subject, that being the period when the constitution is most susceptible of its influence; for the same causes which contribute to originate the disease, also dispose to its reception when once originated.

The Royal Academy of Medicine in Paris having offered a prize of 10,000 francs for the best solution of the following questions, on the subjects of cow-pox and small-pox, subjects to which I had previously directed my attention, I propose to myself to follow that order of investigation.

Question 1.—Is the preservative power of the vaccine virus permanent or temporary?

Question 2.—In the latter case determine, by precise experiments, and by authentic facts, the time during which its preservative power continues.

Question 3.—Has the matter of cow-pox taken from the cow, greater efficacy than the vaccine virus which has been transmitted through several individuals?

Question 4.—Supposing it to be proved that the efficacy of the vaccine virus is diminished by transmission, should it be renewed, and by what means?

Question 5.—What is the relation between the intensity of the local phenomena, and the preservative power of the vaccine virus?

Question 6.—Is revaccination necessary? and if so, point out at what period revaccination should be practised.—*Lancet*, October 13th.

With respect to the first question, undoubtedly the preservative influence of vaccine matter is only temporary, and from the following facts, the inference is good, that the grand epochs in the human body, for vaccination, are childhood and puberty; the susceptibility of children to this disease being rather *common*, as arising from their highly irritable and vascular skin, which disposes to exanthematous diseases, than *specific*, as arising from any peculiar susceptibility; and hence small-pox is much more severe and much more fatal in the adult and at the period of puberty, than either in childhood or old age; indeed the latter, after the period of 45 or 50, enjoys an almost perfect immunity from this affection; this immunity is relative, and not absolute, deviating in a direct ratio with its causes, namely, the earlier or later decay of sexual feeling.

According to Mr. Streeter's statement (LANCET, January 1838), the proportion of cases over or at puberty, admitted into the Small-Pox Hospital, was as 12 to 1; hence that gentleman advocates this view of small-pox, so far as relates to revaccination at puberty. This view seems strongly illustrated by the following cases, which recently fell under my observation in Billiter-square.

I was requested to visit a young lady, aged 21, whom I found indisposed from a slight attack of modified variola. She had been vaccinated in infancy, as well as every other member of the family, which consisted of four daughters and two sons, all unmarried (one young lady was fortunately absent); the disease attacked all present in succession, and they suffered as follows:—The first-mentioned case was slight, there appearing not more than 40 pocks altogether. The second case was that of the eldest daughter, aged 32, who suffered most severely, and for whom I entertained considerable fears as to her recovery, which, however, gradually took place, after the copious administration of port wine, a wine-glassful every four hours, at the period of the maturation of the pustules. The third case was one of the sons, aged 26, in whom perhaps 100 marks were present. The fourth case, that of the other son, aged 17, who

* Hippocrates, Hilary, Mead, Lord Bacon, &c.

offered not more than seven or eight pustules; and with the youngest daughter, 16, one solitary pustule, with slight indisposition, was all that could be diagnosticated of variola.

It was impossible for me not to be forcibly struck with the relative severity of these cases, which was in a direct ratio with the period that had elapsed since vaccination.

The manner in which the first mentioned patient had contracted the disease, was described as follows:—Walking in Grace-church-street, her attention was suddenly attracted to a poor child, whose face presented powerful traces of a very recent attack of small-pox; she was much shocked at the sight, hastened home, chilly and uncomfortable, with a remarkably disagreeable taste in the mouth, and within a fortnight the eruption made its appearance.

In the same way small-pox repeatedly makes its *entrée* into a family. The following case occurred within this month; a respectable maid-servant, vaccinated when young, walking out with children, met a sufferer from small-pox; she immediately felt indisposed, returned home, and within a fortnight modified variola appeared. The family, consisting of husband, wife, and four children, were at once revaccinated, without, however, any effect; but the small-pox did not spread beyond the first individual.

From these cases it follows, *secondly*, that cow-pox preserves from small-pox only up to the period of puberty, and that a second inoculation of small-pox, fresh from the cow, becomes equally, if not more necessary than at the period of childhood; and the solution of *this* question necessarily answers the last proposed by the Academy; since to determine the period of efficacy of cow-pox inoculation, is to determine the period necessary for its renewal.

This construction of facts is strongly corroborated by various papers and reports on this disease: the Small-Pox Hospital Reports of Dr. Gregory, and other communications to that gentleman from different sources and quarters; one from a Mr. Chapman, of India, proclaims loudly that after ten or twelve years' vaccination, its efficacy appeared perfectly nugatory, the epidemic variola attacking equally the vaccinated and the unvaccinated; and it also appears as has previously been noticed, that the female seems peculiarly obnoxious to this affection. Thus it is interesting to notice that the origin of the Norwich epidemic of 1819 was attributed to a travelling girl. From the history of this epidemic, published by Mr. Cross, it further appears that the only cases of small-pox after small-pox occurred after the period of puberty; and more recent experience, I think, bears me out in the assertion that the most severe cases of small-pox after vaccination, are those which have occurred at, or subsequent to, the period of puberty. Now, while, in my humble opinion, cases of small-pox after small-pox will tend materially to elucidate this abstruse subject, I must also candidly aver that cases of small-pox after vaccination, as at present practised, afford no grounds for legitimate induction; doubtless, however, the nearer we approach the period of Dr. Jenner's first introduction of the cow-pox matter, such cases become more valuable; hence Mr. Estlin's account of a discovery of the natural cow-pox (see "Medical Gazette" and Forbes's Quarterly) in which all the milkers (who had been previously vaccinated, one even by Dr. Jenner himself,) were reinfected by the cows, becomes peculiarly interesting, as apparently tending to establish the truth of this theory. Dr. Gregory also states, in the "Medical and Physical Journal," 1826, that the only four cases of small-pox after cow-pox, which he could confidently rely upon, were three young ladies and one young gentleman, whom he was called upon to visit in private practice. The reports from France, Prussia, Austria, Italy, Norway, Sweden, Iceland, Ceylon, the East and West Indies, all agree in stating that between the periods of fifteen and twenty years after the introduction of vaccination the small-pox resumed much of its former severity.

Thus vaccination was introduced into the following countries within 1800 and 1802, and after an extraordinary immunity from variola, that disease reappeared in the following years:—

France	1816
Holland	1818
Germany	1819
America	1815
West Indies	1819
Ceylon	1819

To this list I beg to add the 18th conclusion of Dr. Thomson, in his work on small-pox:—

"That the attacks of epidemic small-pox, which have spread themselves so extensively over the globe *since the year* 1814, prove beyond possibility of doubt, the occurrence of secondary small-pox after vaccination."

And to the laborious and learned appendix of that volume, I must beg to refer the reader for the most extraordinary illustration of the truth of the principles here inculcated, when this theory was as yet unborn; for the narration of some hundreds of cases of secondary small-pox after small-pox, and of small-pox after vaccination, all (with few exceptions, and those *invalid*) tend to establish the doctrine, that cow-pox, and natural small-pox, both exercise a preservative power, against re-infection by small-pox only up to, or a little beyond, the period of puberty, and with this accumulated evidence, I conclude this division of the subject. It should here, how-

ever, be observed, that I agree in opinion with Mr. Cross, and others, in this, that inoculated small-pox, when unaccompanied by an eruption, should be considered *imperfect*.

It follows, thirdly, that cow-pox, fresh from the cow, is more efficacious than vaccination, as now employed.

It is also equally clear, from facts universally admitted and recorded, that the vaccine lymph, in general use, has ceased to exercise that control over small-pox, which it formerly did.

Fifthly. The degree of local inflammation should be purely *specific*, as arising from the introduction of cow-pox matter; and not at all *common*, as arising from a morbid irritant; the inflammation should be phlegmonous and not erysipelatous, defined and not diffuse, with a slight access of fever between seven and eight times, twenty-four hours after vaccination; it must be observed that all these marks are relative and not absolute, for summer and winter, or heat and cold, which are both opposed to the production of small-pox, also diminish the susceptibility to this disease. Hence Dr. Jenner's golden rule "never to use the virus after the formation of the efflorescence around the pustule."—(See "Letter to Dr. Lettsom.")

If then, as follows from the preceding remarks, the frequent reproduction of cow-pox be requisite for the full efficiency of its preservative power, the question by what means we can effect this, becomes, indeed, vitally important; and here I must observe, that I have nothing far-fetched and visionary to offer on this particular head; to me it appears that having traced out the causes of cow-pox as occurring—

First, in the breeding female animal, being separated from its young;

Secondly, its occurring most commonly in spring;

We must be content to "imitate Nature." This view is strongly supported by the fact, that when occurring in counties where such causes do not commonly exist, it is the consequence of an accidental occurrence of the causes, such as the early death or separation of its young, an instance of which fell under my own observation when recently making a visit in a non-dairying county.

Subject to the test of experience and further facts, I deduce from the foregoing:—

First, and principally, that the opinion that small-pox has had but one individual and solitary origin, whence all mankind became infected, is most illogical; that it is founded on our *ignorance*, and not on facts; that it is negative and not positive; that it is opposed by daily experience of the phenomena of this disease, its rise, its progress and decline, its frequent occurrence where no contagion can possibly be traced, and its deviation in a direct ratio with the causes which produce it; that it is also opposed by the foregoing evidence of its identity, in causes and nature, with cow-pox, which latter disease originates *de novo* frequently, &c. &c.

Secondly. That cow-pox and small-pox, are identical—

1. In their causes.
2. In their nature (subject to the law of physical condition).
3. In their effects.

Thirdly. That the circumstance of one being a local, the other a general exanthematous affection, is the result of the different anatomical construction of the two animals in which they originate, according to the law which governs the actions of all specific poisons.

Fourthly. That cow-pox, fresh from the cow, is more efficacious than vaccination as now employed.

Fifthly. That the preservative power is temporary and not permanent, preserving the constitution only up to the period of puberty.

Sixthly. That cow-pox, fresh from the cow, and natural small-pox, exercise identical preservative influence over the occurrence of secondary small-pox.

Seventhly. That the two grand epochs for vaccination are childhood and puberty.

Eighthly. That the inference that the vaccine matter has weakened by time, is by no means a necessary, or even a fair logical induction, since there is considerable foundation for the opinion that, as all other epidemics are constantly altering their type, deviating in a direct ratio with their causes, the small-pox also does the same.

Ninthly. That the method of reproducing cow-pox consists in bringing the animal under the influence of the same circumstances which are peculiar to the locality of its origin—the dairy counties; namely the early separation of its young.

Pathology of Dogs.—(*From a Correspondent*).—Dr. Forster, late of Cheltenham, has published, in Belgium, a tract on "Canine Pathology," with a view, in particular, to clear away what he regards as the prejudices of the medical world respecting hydrophobia, which he considers that he has proved to be a very rare disease, and consequently often alarms the world unnecessarily, through the means of newspaper paragraphs. He has also made some curious observations on cases of *neurosis* in dogs, and has given a long list of fancy and pet dogs which had been well fed, with the respective longevity of each, compared with the longevity of hardy shepherd's dogs, to prove that high feeding is the chief cause of the premature decay of the dog. He has concluded his memoir with the biography of a singular dog of his own, whose instincts and affections were of the highest order amongst animals of the canine race.

WESTMINSTER HOSPITAL.

COMMITTEE OF INQUIRY

INTO THE CONDUCT OF

SIR ANTHONY CARLISLE,

IN THE TREATMENT OF

THOMAS HOLMES,

Held on Friday, October 19th, 1838.

A SPECIAL meeting of the House-Committee took place at the time, and for the purpose above stated, appointed by the Weekly Board which sat on the 9th instant, and whose proceedings have already been noticed in this Journal. This court of inquiry was to consist only of members of the House-Committee, but all Governors were at liberty to attend, and to mix in the discussion, although they have, on these occasions, no power of voting. There were between twenty and thirty persons present; of these the following were members of the House-Committee, and constituted the tribunal before whom Sir Anthony's conduct was to be tried:—Mr. Bicknell, Mr. Green, Mr. Ayrton, Dr. Brookes, the Rev. H. H. Milman, Rev. Cyril Page, Dr. Wright, Dr. Bright, Dr. Burne, Sir A. Carlisle, and Mr. Guthrie.

The reporter does not undertake to record the *copia verborum*, which emanated from the speakers on both sides, he merely pretends to communicate the substance of what each gentleman said, so far as the conversational character of the discussion, the frequent interruptions, and the general irregularity of the proceedings permit. At a quarter past one o'clock Mr. BICKNELL took the chair. The Chairman addressed the meeting in a neat speech, and recommended the temperate and impartial discussion of the subject. The charge brought against Sir Anthony Carlisle was a most serious one, affecting not only the judgment and character of the senior surgeon, but also the welfare of the institution. He should endeavour to maintain the authority of the Chair with firmness and fairness, and he trusted the meeting would support him in his efforts.

In the absence of Mr. J. C. Wood, who was detained in the country by pressing business, Mr. CHEERE opened the proceedings, by stating that in pursuance of the notice contained in the resolution of the Weekly Board of the 9th instant, he laid on the table three numbers of the medical publication called THE LANCET, in which were contained certain charges of neglect and incapacity against the senior surgeon. He was sorry to say that he believed the charges to be true, both because his private inquiries had led to a corroboration of the statements in THE LANCET, and because those statements, from their great clearness and succinctness, bore innate marks of being genuine. He had no feeling of hostility against Sir Anthony; on the contrary, he respected his years, his professional rank, and his attainments; but he felt that he should not be acting with independence, or justice, if, whilst he was active in punishing the delinquencies of the inferior officials of the charity, he were to shun the odium of inquiring into the authenticity of charges against the chief of the medical staff. He was ready to adopt any mode of inquiry the meeting might deem proper, but as a preliminary, he should move that the minute of the Weekly Board, relating to the subject be read, and he should place the three numbers of THE LANCET on the table, for the perusal of the gentlemen present.

The following minute of the Weekly Board of the 9th October was then read; it ran as follows:—

Resolved,—That an especial meeting of the House-Committee do take place on Friday, the 19th of October instant, to make inquiry into the treatment of Thomas Holmes, by Sir Anthony Carlisle, and that Sir Anthony be requested to attend.

After some discussion as to the proper mode of proceeding, the Chairman, the Rev. H. H. Milman, and Mr. Harrison, objecting to entertaining the anonymous statements in a journal as grounds of charge against the conduct of an officer of the establishment, the difficulty was got over by Mr. Cheere's adopting the statements of THE LANCET as his own, and undertaking to prove their correctness, by examining those medical officers of the establishment who had had the care of the case.

Sir ANTHONY CARLISLE now came forward, and stated, that having received a notification from the Secretary, that an inquiry was to be instituted into his conduct regarding the treatment of Thomas Holmes, an inquiry based upon anonymous charges, he hardly knew whether he was to consider himself in the light of an accuser, or of an accused person; of an accuser of the malicious and anonymous slanderer who had unjustly assailed him, or as one accused of neglect of his duty towards one of his patients. He should not allude more largely to the source of the accusation, but he would beg to read to them a statement of the case of Thomas Holmes, the poor man concerning whom he had written a *provisional* certificate. Sir Anthony now read his statement, intermingling various comments with the text, as he proceeded. The essence of the address was as follows:—

Thomas Holmes, an elderly man, addicted to drinking, was admitted under my care on the 22nd of August last, with an injury to the bones of the forearm, above the wrist. The man's employer gave him a good character, and was anxious to have him restored to his service as soon as possible. He

was taken care of by Mr. Lynn, who is kind enough to attend for me in my absence, and by the house-surgeon. I saw him, for the first time, on Saturday, the 25th of August, and found him labouring under an attack of "delirium tremens," or "drunkard's delirium," a "shaking palsy," as it has been named by different authorities. Having always found opium to be a more eligible medicine in this disease than stimulants, I ordered that he should continue the opiate remedies, which Mr. Dasent had very properly ordered for him. I had no doubt in my mind, that the case was the common one of "drunken lunacy," to which the mechanics and labourers who drink spirituous and malt liquors in great quantities are liable. I was led to the opinion by the fact, that he had a striking delusion that a man was perpetually staring at him. He was also very strong in the *arms* and *legs*, as well as insensible to pain, all which circumstances are characteristic of lunacy. I saw him again on Tuesday, when he was in a quiescent state, due, as I conceived, to the influence of the forty drops of laudanum which had been administered to him on the previous night. I approved of the step which the Governor, Mr. Bicknell, had taken, with the sanction of the house-surgeon, of having him placed in a room by himself. Being informed that he had for two days and nights broken the rest of his fellow-patients, I left a *provisional* certificate with the Secretary, which, in case his symptoms should a second time become violent, might be acted upon by any of my colleagues, by countersigning the certificate. Fortunately, however, the paroxysm did not return, and as is not unusual in "drunken mania," the patient apparently recovered. He has since that time been under my care, and is perfectly quiet; the affected forearm, however, has been materially injured by the violence of the patient during the paroxysm of mania. I shall not refer at length to any services I may have rendered formerly to this institution; they were not unknown to many who now rest in peace. I may, however, without vanity, perhaps, declare, that I was the founder of the Building Fund, the accumulation of which has enabled us to erect this handsome edifice. I laboured for years in that undertaking, not only unassisted, but actually bitterly opposed by my colleagues, who ridiculed the notion as Utopian. I am not unacquainted with the legal as well as medical points connected with insanity. For thirty-eight years I have medically superintended the asylum of Sir Jonathan Miles, at Hoxton, and have constantly been in the habit of treating maniacal as well as delirious cases. Cases of "drunkard's mania," or "shaking palsy," I have had innumerable opportunities of witnessing; and, of course, this frequent commerce with these two classes of malady, has enabled me to obtain powers of discrimination not possessed by the generality of medical men. I have attended my duties regularly ever since I was first attached to this charity; I believe I may say, without offence, more regularly than any of my colleagues. That I have not been so assiduous in my duties as trustee, is due to the advice of an excellent friend of this institution, who told me, some years ago, that the medical men made themselves too busy in the affairs of the hospital. I willingly yielded consent to his suggestion. I have derived no personal benefit by exercising the functions of trustee; I exercised it only for the benefit of the charity, and when I thought my retirement from the Boards and Committees more agreeable to the Governors, I cheerfully absented myself. I shall leave the entire case in the hands of the Committee, being fully satisfied they will do me justice.

As soon as Sir Anthony had finished his address he retired, though he was urgently requested to stay by his friends and opponents generally, and by Mr. Cheere especially.

A tedious discussion now took place as to the proper steps to be taken in the prosecution of the inquiry.

Mr. Milman, the rector of St. Margaret's, thought that some distinct charge should be made and embodied in a resolution, which should be moved and seconded, and thus subjected to the decision of the meeting. Unless there was some motion before the Committee, he did not see how they could proceed.

In consequence of these observations, Mr. Cheere handed up to the Chair a resolution, which was couched thus:—

Resolved,—That it is the opinion of this Committee, that Sir A. Carlisle was not justified, by the circumstances of the case, in writing a certificate of insanity against Thomas Holmes; and further it appears to this Committee, that Sir Anthony Carlisle neglected the said Thomas Holmes, in the progress of the treatment.

Mr. Cheere now requested that the house-surgeon might be called in, and questioned; and, with a view to insure the regularity of the process, he wrote down a number of queries, which he requested the Chairman to propose to that functionary. Mr. Bury Dasent gave his evidence in a very clear and unambiguous manner; in addition to the queries of Mr. Cheere, questions were directed to him by the Chairman, and by several other gentlemen, and the substance of his replies is contained in the following statement:—

He recollected the case of Thos. Holmes; he was admitted on the 22nd of August last, with an injury to the wrist-joint, received at the gas factory; he was admitted at the instance of Mr. Lynn, and was placed under Sir Anthony Carlisle, as the surgeon of the

week. The day of his admittance was Wednesday. Mr. Lynn prescribed fomentations to the injured arm, and did not see him afterwards. He was not seen at all by Sir Anthony Carlisle till Saturday, the 25th, when he was suffering from delirium, not very furious delirium. The man became delirious, but only slightly, on Friday evening; he told him (Mr. Dasent) that he saw a face staring at him; he knew it was a delusion, he said, but could not get rid of it. He (Mr. Dasent) gave him a dose of morphia, which gave him a good night. Sir Anthony did not see him again till Tuesday, the 28th of August, when the man was lying in his bed, exhausted, but quite rational. Sir Anthony looked at him, said the man must be kept quiet, and approved of the treatment which had been adopted. He (Mr. Dasent) had prescribed opium, in the first instance, on his own responsibility; he prescribed for the patient ammonia and opium about ten o'clock on the forenoon of Saturday, the 25th of August. A little past twelve that day Sir A. Carlisle first saw the patient, was informed of what medicine had been given, and ordered it to be continued. He (Mr. Dasent) in the course of the afternoon saw that it was expedient to leave off the ammonia, but he continued the opium. All this he did of his own judgment; he treated the case during the whole period of the paroxysms of delirium, but he had had the sanction of Mr. Hale Thomson, the assistant-surgeon, for the mode of treatment he resorted to. Holmes was very violent, and attempted to escape. It was necessary to put a straitwaistcoat on him, and to place him in the operation theatre, in order that he might not disturb the other patients. The delusion regarding the face did not last after the first night; the next day, though more violent, Holmes did not recollect the delusion of the previous evening. He had not slept from Saturday morning till Monday night. On Friday night he slept well after the opium, and on Monday night (the 27th of August), he also slept soundly, and awoke on Tuesday morning convalescent,—yes, PERFECTLY convalescent. When Sir A. Carlisle saw him on that day he was convalescent. Sir Anthony did not ask him any question; he made no inquiry into the previous history of the case. In his (Mr. Dasent's) opinion the case was one of delirium tremens. There was the tremulous motion characteristic of that disease, but not in a very high degree. His wife said that he was a hard-drinker, and that he had had two attacks of the same kind before, but not after any injury. He (Mr. Dasent) was a member of the Royal College of Surgeons. He had been for nearly two years house-surgeon to the hospital, and thought himself competent to treat cases of this description. He had seen several cases of delirium tremens, and one well-marked case of mania in the hospital. He felt no difficulty in diagnosticating between the two. He still considered himself as a learner, and was happy to derive information from any quarter. He did not send to acquaint Sir Anthony of the occurrence of the delirium on Friday, because the day following was Sir Anthony's day of attendance, and because the case was known to Mr. Lynn and Mr. Thomson. In serious cases it was usual to send to the principal surgeon and to the assistant-surgeon, but it was Sir Anthony's desire, that when any of his cases were critical, he (the house-surgeon) should send for Mr. White or Mr. Lynn, who resided near the hospital, and who generally attended for him. The man had not been sent away, according to the command of Sir Anthony, but remained in the ward, *perfectly sane*, and was now treated solely for the injury to the wrist-joint. He was still under the care of Sir Anthony, who approved of the contravention of his own order. Sir Anthony was always eminently kind to the patients.

Mr. Dasent having withdrawn, Mr. Wilson, the Secretary, was questioned as to what he knew of the transaction, and supplied the following information:—

The certificate of Sir Anthony Carlisle, asserting the insanity of Thomas Holmes, and counselling his removal from the hospital, being received by the Weekly Board, sitting on Tuesday, the 28th of August, 1838, thereupon the following resolution was passed:—

Resolved,—That the Secretary do take *immediate* steps to remove Thomas Holmes from the hospital.

It was his duty, as Secretary, to act forthwith upon this resolution, and to take steps for the immediate removal of the poor man from the house, as his presence was stated to be injurious to the other patients; but he was arrested in his proceedings, by a remonstrance from the assistant-surgeon and the house-surgeon, who declared that the certificate was most unwarrantable, and that the patient Holmes was as sane as any man in the house. Unless the sanction of these medical officers had authorised his non-fulfilment of the Board's order, he had no discretion or option, but to turn the man out of the hospital; and he had, in fact, given notice to his friends, that they might fetch him.

The CHAIRMAN now craved leave to state, that he was a member of the Committee that received the certificate of Sir Anthony, and perfectly remembered some of the circumstances. As a house-visitor, he had entered the ward in which the patient in question lay, and, witnessing his violence, he recommended his removal into the theatre. Conceiving the man to be insane, and, according to the tenor of Rule 44, unfit to remain in the hospital, he spoke to Sir Anthony on

Tuesday; that gentleman concurred in the opinion that Holmes was mad, and unfit to remain in the house, and at his (Mr. Bicknell's) request, wrote the certificate, which had furnished the subject of the present discussion. He did not pretend to be a judge of the nature of the mental affection, but as the patient had been violent for three days, his remaining any longer in the institution appeared to him inconsistent with Rule 44. The certificate given by Sir Anthony Carlisle was not a provisional one, but intended to be acted upon forthwith by the Board, and the Board had acted accordingly.

Mr. HALE THOMSON, in reply to some questions of Mr. Cheere and Mr. Edwards, stated, that he had seen the case alluded to, but had not treated it, in consequence of an injunction to that effect from Sir Anthony. He had simply supervised the case, which had been admirably treated by Mr. Bury Dasent. In his opinion the case was clearly one of well-marked delirium tremens, and he should not, in any stage of the treatment, have given a certificate such as that of Sir Anthony.

The certificate of Sir Anthony was read.

The CHAIRMAN now stated, that all the facts of the case being elicited, he should be happy to hear any observations which the gentlemen present might be disposed to make.

The discussion was rather desultory, but the substance of the communication of each speaker is accurately given in the following summary:—

Mr. AYRTON said,—Mr. Chairman, having been an active Governor of this institution for thirty years, I feel myself called upon to bear my testimony to the zeal and ability with which Sir Anthony Carlisle has served the hospital, as well in the capacity of Governor, as in that of a most distinguished medical officer. I have listened with the most careful attention to the whole of the evidence that has been brought forward by the promoters of the inquiry, as well as to the luminous statement of Sir Anthony Carlisle; and I must avow my conviction, that I do not see the slightest ground whereon to maintain the charges against the accused. So far from any blame being due to Sir Anthony for his conduct in this case, I think he has claims to our gratitude for the prompt humanity with which he stepped forward to relieve the patients from the annoyance and fear which the presence of a maniac excites, and the poor lunatic from a situation in which the best means for his recovery could not be applied. The senior surgeon attended regularly at the time appointed for him; he saw the patient, ascertained and approved of the treatment that had been adopted for him, and finding the delirious symptoms very violent, he wrote a *provisional* certificate, which was to be carried into effect whenever the maniacal paroxysms should become urgent. The certificate which Sir Anthony wrote, he was fully aware, could not legally be acted upon by any keeper of receptacles for the insane, without a second signature, and the writer did not contemplate the addition of that other signature being effected, unless some more decided traits of the man's disease should render it expedient. I think, therefore, the serious charge conveyed in the resolution of Mr. Cheere, is not based on the facts of the case, and I declare it to be my deliberate opinion, that "the bill ought to be ignored."

Mr, LYNN thus spoke:—Thomas Holmes was admitted into the hospital at my instigation; the injury of the wrist was not in itself of sufficient importance to justify his admission, but there was something about the general appearance of the man which warned me of the occurrence of some subsequent constitutional mischief. I prescribed a fomentation for his arm, and I did not see him afterwards. I have anxiously attended to the evidence and the statements adduced on both sides. My attention has been particularly arrested by the fact, that the man has had two previous attacks of mental aberration not arising after an injury. This circumstance, and the reasonings delivered in Sir Anthony's speech, satisfy my mind that the case was one of insanity, and that consequently the certificate was a justifiable document.

Mr. GREEN said, I must confess, from the most careful consideration I can give to the subject, that I do not see in the conduct of Sir Anthony Carlisle, in this business, any justification of the strong censures conveyed in Mr. Cheere's resolution. It is obvious, from the questions propounded by several medical gentlemen to Mr. Dasent, that no unanimity of opinion exists amongst them as to the nature of the case, or as to the distinguishing marks of delirium tremens and mania. How, then, can this Board, consisting of unprofessional men, decide what is the difference between the two affections? It appears to me that we cannot, with propriety, enter into this subject; it is a nosological question, which medical men should settle amongst themselves. I cannot concur in the opinion, however, that the issuing of the certificate was a prudent or necessary step; and I do not think that the senior surgeon, although he performed his duty to the letter of the regulations, paid that careful attention to the progress of the case which its very serious nature seemed to demand. I cannot concur in the severe motion of Mr. Cheere, neither can I entirely exonerate Sir Anthony from blame. The censure which I think is merited in this case would, in my opinion, fall equally upon the shoulders of all the medical officers, who are in the habit of surrendering the cases too indiscriminately

to the care of the junior officials. The out-patients, I believe, are almost entirely left to the house-surgeon and clinical clerk, who, however talented they may be, are not contemplated by the Governors as the responsible medical officers of the institution. Any resolution affecting Sir Anthony alone, would, I think, be partial and unjust.

The Rev. H. H. MILMAN said, I coincide in opinion with the gentleman who has just spoken, as to the incompetency of this Committee to decide upon the nosological distinction between delirium tremens and insanity. I feel myself quite incapable of forming an opinion on that point; and if it be the intention of this meeting to decide the question, I shall most certainly withdraw. It appears to me that the proper business of the day is, to determine whether Sir Anthony Carlisle was justified by the circumstances of the case, so far as they can be appreciated by us, in writing a *provisional* certificate for the removal of the patient. We have evidence that the patient was extremely obstreperous and unruly, disturbing the sleep and comfort of his fellow-sufferers; and we have the opinion of his attendant that this state of things was likely to continue. If it was the opinion of the surgeon, as it obviously was, that this was a case of insanity, and therefore more fitted for the moral treatment of a retreat for lunatics than for the medical discipline of a general hospital; and further, that the continuance of this man under the hospital roof was absolutely injurious to the other patients, I think we must exculpate the surgeon from any blame, and admit that he could not, consistently with his duty, refrain from taking the steps he did for having him removed. It must be remembered, also, that the certificate was a *provisional* one; and though, perhaps, it might have been better for Sir Anthony to have left word with the house-surgeon to acquaint him with any important change that might take place in the patient, in order that he might *then* have acted, I cannot but acknowledge that the visiting such an error of judgment with a severe censure would be revolting to my sense of justice and fairness.

Mr. D. O. EDWARDS,—Having become, in some degree, the unwitting cause of this inquiry, by being the author of the history of the case of Holmes, published in THE LANCET, I feel bound to add my testimony to the evidence already adduced in support of the charges against Sir Anthony Carlisle. The history of the case which has appeared in that Journal was drawn up conjointly by my friend Mr. Dasent (the house-surgeon) and myself. Our only object in publishing the case was to preserve an instance of the efficacy of large doses of opium in the treatment of delirium tremens. When the case was written out for the printer, one of the Governors brought me a copy of the certificate of Sir Anthony Carlisle, which has been already read, and he stated that I should not publish the whole truth if I omitted that certificate; it was, therefore, appended to the case without comment. It is scarcely necessary to assert, that the history of the case published in THE LANCET, and forming the ground of the accusation against Sir Anthony, is accurately correct, as it has been confirmed by the evidence given at this Board. I have been actively engaged in the practice of my profession for twenty years, and have resided five years in the hospital. I have had the good fortune, during my professional career, of witnessing many cases of delirium tremens, as well as of insanity, and I think I may venture, without presumption, to give an opinion upon this subject. It is my conviction, Sir, that no two diseases in the nosology are more distinctly characterised than delirium tremens and insanity. The distinction is complete, whether we regard the outward symptoms or the character of the hallucination; the outward symptoms are so extremely well marked, that any tyro that has seen two cases can never after mistake the disease. The peculiar tremulous action of the muscles, the great and capricious inequality in the force and character of the pulse; the remarkable and—if I may use the expression—the idiomatic character of the countenance, and the duration of the affection, equally distinguish the disease from mania and from simple febrile delirium. The character of the hallucination is not less emphatically distinct than the diversity of symptoms is remarkable. In every form of insanity, the delusion, or delusions, which constitute the essence of the malady, are constant and unchangeable; the morbid association, which the intellect is unable to shake off, whenever presented to the imagination, invariably produces a paroxysm, however often repeated, or at however long intervals. But in delirium tremens and febrile delirium there is no constant delusion which thrusts itself upon the subjugated understanding, and haunts the imagination like a shadow; the delusions are perpetually changing, and immediately forgotten; the most pertinacious of these delusions scarcely lasts an hour, and if one attempts to renew them, the effort is always futile. In mania, the false association is independent of febrile action, and perfectly chronic in character. In delirium, the delusions are the results of excessive action of the brain and nervous system; ideas are formed in the mind with such excessive rapidity, that the attention is perfectly unable to arrest the whole series; an idea here, and an idea there, are linked in the memory, and the incoherent conception, which is the result, indicates the disturbed condition of the machinery. It is exactly as if, in reading, the eye ran so rapidly over the lines as to be unable to catch more

than every fourth or fifth word; these terms, when united, would, in consequence of the extensive ellipses, represent no coherent notions. In fact, I do not hesitate to say that the physical and moral lines of demarcation in these two diseases are so prominent, that the medical man who should mistake the one for the other would be inexcusable. I have carefully watched the case of Thomas Holmes, and I visited him several times during his delirium, and afterwards. I never have seen a more unambiguous case of delirium tremens. The delusion of the man's face staring at him did not last after the first evening; and I stoutly deny that this spectrum is, as Sir Anthony Carlisle states, a mark of insanity; had it been constant it might be so considered, but it was not constant, as appears from Mr. Dasent's testimony, and my own observation. The statement of Sir Anthony Carlisle shows more clearly than any other evidence that he mistook the nature of the case. The distinctions he draws between the two maladies are not the real ones, for the symptoms noticed by him belong to both; the strength is preternaturally induced in delirium as well as in mania, and in both there may be a delusive object, though distinguished in the way I have pointed out. I have stated my opinion thus boldly, because I perceive in the Board a backwardness in entering into the merits of the case, and because I know every medical man who hears me does, in his conscience, know that what I assert is true. The charge against Sir Anthony Carlisle is, that he neglected to make a proper inquiry into the case; had he informed himself of the previous history, and carefully examined the symptoms and the nature of the aberration, he could not have allowed himself to sign a certificate which directed the expulsion of the patient at a critical moment. The certificate was not simply provisional, it was unconditional as regarded the removal of Holmes from the institution, a step highly dangerous to the poor man; that it was not adequate to ensure the admission of the patient into a lunatic asylum, was an aggravation of the evil, and no apology for Sir Anthony Carlisle; the question we have to determine is, the fitness or unfitness of Sir Anthony to perform the duties of surgeon in ordinary to this establishment, as exemplified in the treatment of the case under consideration. *(Cries of "No, no.")* At all events such a question is a direct consequence of the present investigation.

Mr. HARRISON said, although I cannot boast, like the Gentleman opposite, who has favoured us with a dissertation on delirium and insanity, of having been entirely engaged for twenty years in the practice of my profession, nor of having had the great advantage of five years' residence in the institution, yet I have resided and studied at medical establishments of larger dimension and equal celebrity. I have, during my career, witnessed the treatment of many cases of the shaking palsy and of mania; and I have had opportunities of watching the progress of the case under consideration, and I do not hesitate to state, in contradiction to the dogmatic assertion of the learned Governor, that to distinguish between delirium and insanity is not so easy a task; and, further, that the case of Holmes was one of decided insanity. If the being subject to mental delusions, and believing in the unreal suggestions of the fancy; if the excitation of the passions be not insanity, what is? The characteristic tremor was not, according to the evidence, very well marked, and in this respect there is a discrepancy between Mr. Dasent and the worthy Governor. Since this gentleman has avowed the authorship of the case in THE LANCET, we may admit it as genuine evidence, and I do not scruple to assert, that the case, as related in that Journal, furnishes conclusive evidence of the disease being insanity. You have there all the marks of insanity which a great legal authority, Mr. Chitty, represents as constituting an exemption in law from the consequences of crimes. "If a man," says that author, "be, from any cause, delirium, insanity, idiotcy, or any other mental aberration whatever, incapable of guiding his own actions, he is *non compos*, a lunatic, legally unfitted to manage his own affairs, and exempt from responsibility as regards the consequences of his conduct." I contend, therefore, that Sir Anthony Carlisle has shown great discretion as well in the moral and legal as in the medical treatment of Thomas Holmes, and I can only wonder that any man, who has observed the case so carefully as the author of the history in THE LANCET must have done, could possibly conceive it to be a case of simple delirium tremens, or shaking palsy. The writers, from Cullen downwards, afford no justification for the assertions touching the facility of distinguishing these diseases. The descriptions are extremely unsatisfactory, and constitute a very sufficient apology for the doubts of any man in cases of this description. I must, upon the whole, therefore, clearly declare my opinion, that Sir Anthony deserves thanks rather than censure at the hands of this Committee.

Mr. LYNN replied, the more I enter into the circumstances of the case, the more convinced I feel that Sir Anthony is right. My friend, Mr. Edwards, has not accounted for the two previous attacks of insanity which the patient had suffered, and he has left out of his consideration the frequent instances in which delirium tremens terminates in madness. I have not seen the case, but I am firmly of opinion that Sir Anthony is right in his diagnosis.

Mr. Edwards, I regret exceedingly, Sir, that circumstances have placed me so forward in the present inquiry. I did not wish to take a share in an inquisition against an old colleague. I have no feeling of hostility to gratify, and I am as willing as any man to testify my respect for Sir Anthony on a proper occasion. A desire of bringing out the truth has alone induced me to come forward, and if the medical officers had honestly declared their opinions on the case I should have been spared the painful duty. In reply to Mr. Lynn I have to observe, that neither bodily injury, nor even habits of inebriation, are indispensable precursors of delirium tremens. I have had three cases in the respectable ranks of life in which delirium tremens occurred as the consequence of mental anxiety; all these cases were treated with opium and recovered. In one of these cases I had the valuable assistance of my friend, Dr. Roe, who is present, and who, I am sure, entertains no doubt as to the nature of the case of Thomas Holmes. The previous attacks of Holmes were no more attacks of insanity than the present one, and furnish no argument in favour of Sir Anthony's view of the matter. Every medical man of any standing must be familiar with instances in which successive attacks of delirium tremens have occurred. That successive attacks of this malady should terminate in madness by exciting the natural predisposition of some constitutions, is no proof of the identity of delirium tremens and insanity. As well might it be said that an egg is not an egg because it may afterwards become a chicken. I have no wish to set up myself as a standard of medical opinions, as insinuated by Mr. Harrison; my sole object is to testify to the truth.

Dr. Burne delivered a forcible address, which made a profound impression on the Committee. We regret that want of space renders us incapable of doing it the justice which it deserves. He proceeded as follows:—I believe it will readily be granted me that no class of diseases treated of in nosography are so difficult to be distinguished from each other, or so little understood by medical men, as the class of nervous diseases, attended with aberration of mind. Our most celebrated practitioners, and our most approved writers, are at variance with each other on many points relating to the diagnosis and treatment of these maladies; but all concur in admitting the great difficulty of treating the subject satisfactorily. Extensive as my experience has been in these diseases I must confess that I never feel so much doubt in my diagnostic powers as when called to a case of this description. The writers on the various kinds of insanity give us very unsatisfactory advice and information regarding the affections of the nervous system; and I hold in my hand an extract from the work of one of the most eminent physicians on the insane, Dr. George Man Burrows, in which he confesses his utter inability to make a satisfactory definition of the respective limits of delirium and mania. If a distinguished writer and practitioner, who has devoted the attention of his whole life to the study of this class of diseases, is thus embarrassed, what can we expect from the judgment in these matters of the medical profession in general, to whom no opportunity whatever is afforded of acquiring a knowledge of mental diseases? I think, Sir, you will agree with me that it is a lamentable fact that the public receptacles for the insane are not thrown open for the reception of students. How can the public expect that a competent knowledge of these most important of all diseases can be acquired, unless opportunities of seeing cases, and studying the moral and medical treatment of them, are afforded? I must confess it appears to me a most unaccountable infatuation that these assemblies of the insane; these only museums where examples of the maladies can be seen, should be bolted and barred from the inquirer, as if a knowledge of this branch of science could be acquired by intuition, or from a contemplation by the mind of its own infirmities. Whilst this ignorance on the subject remains amongst medical men we need not wonder that diversity of opinion prevails, and that men are equally decided on both sides of a question, both sides being, perhaps, equally away from the truth. I have paid particular attention to the evidence given so faithfully and impartially by Mr. Bury Dasent, and I find in it sufficient reason to make me doubt whether the case of Holmes was a case of delirium tremens. I perceive from Mr. Dasent's account that opium was given to the patient on the Friday night, and that a sound sleep succeeded the administration of it; but on the following morning the patient awoke worse than ever. Now, I believe it is a principle in all cases of delirium tremens, that the production of sleep is the first step of convalescence, and that the patient invariably mends from that change. In this instance, no convalescence followed the first sleep induced. It might fairly be inferred, then, that the disease was insanity, from the absence of that feature of delirium tremens. When the second sleep occurred, on Monday night, also after the exhibition of an additional dose of opium, it might naturally be expected, that the quiescence which occurred was of a deceptive character, the mere effect of the narcotic, and that the paroxysms of insanity might break out at any moment. I think, therefore, that Sir Anthony might very easily be induced to suppose that the calm was only temporary, the result of exhaustion and of opium, arising from the unceasing agitation of the patient. But while I throw out these suggestions, in the hope of

impressing the Board with a true conception of the difficulty of deciding a question of this kind, I do not pretend to give an opinion upon the case itself. I do not say it is delirium or the contrary. I must, however, avow the regret I feel that these serious charges should be brought against my esteemed colleague, Sir Anthony, whom nobody respects more than I do, with such indiscreet if not indelicate zeal, and upon grounds so indisputably trivial. With regard to the certificate which Sir Anthony has been induced to indite, I must confess that I feel great doubts as to its propriety; whether the case was delirium tremens or not, a little time might have been given to the patient to furnish him with a chance of recovery in the hospital. I think the order which Sir Anthony gave for the man's dismissal quite unwarrantable. But, Sir, while we admit that the profession at large, and that even the most distinguished medical philosophers are puzzled in their dealings with nervous and mental diseases, why should we suppose Sir Anthony Carlisle to be an exception to the general rule? Having thus fully expressed my opinion on this particular case, I feel myself called upon, as a physician to this establishment, to declare publicly, that I have long and strongly felt that my admirable colleague, Sir Anthony Carlisle, though an ornament to his profession in the last century, is now rendered, by advanced years and growing infirmities, perfectly unfit for the due performance of the duties of his office in this establishment. He is not only incapable of performing any capital operation, but even any ordinary operation, without danger to his patient. (*Cries of "Order, order."*) He is not only now incompetent, but he has been so for the last ten years, and his daily increasing imbecility and senility, render him more and more useless.

Mr. AYRTON said, Mr. Chairman, I must call the hon. physician to order. I never before heard a more insidious and unfounded attack from one colleague against another. If the learned physician has any charges to make against Sir Anthony Carlisle, let him bring them manfully forward and they shall be met.

The CHAIRMAN—Mr. Ayrton, I must call you to order for using the word insidious; you must recal it.

Mr. AYRTON—Sir, I made use of the word advisedly, and I will not recal it. I never witnessed a more uncalled-for, a more insidious, attack.

Dr. BURNE—I made no insidious attack. I merely made a straight-forward statement. I must continue to observe, that it is a great pity that old, men who have survived their mental powers, should pertinaciously adhere to situations of great responsibility, regardless of the mischief which their imbecility may produce. It is to be lamented, Sir, that young men, in the flower and vigour of their age, and fitted by their talents to uphold and adorn society, should be kept in the back-ground, and restrained from exercising public functions, by the insatiable cupidity of vain senility. The speaker was now silenced by universal calls to order, in which the Chairman joined.

Mr. MILMAN said,—I think we are wasting our time in listening to these medical dissertations. We are not a tribunal of critics to decide upon these nice points of diagnosis; we must apply our minds to the common sense of the case, and decide without reference to the medical opinions.

Mr. CHEERE said,—Having brought forward these charges, which excite great and general interest, as the *locum tenens* of Mr. J. C. Wood, I think I ought to say that the investigation has not altered my opinion of the validity of the accusation against Sir Anthony. I have no feeling to gratify, one way or other, except a love of equal justice. I shall merely state my impressions to the Committee; it will be for them to say if they are well founded or not. If they come to a decision that Sir Anthony remains blameless, I own I shall be very much surprised; but if they are satisfied, so shall I be. Are they satisfied that Sir Anthony Carlisle did his duty in not seeing this most important and dangerous case more than twice during the critical period of the treatment, and the second time was after the danger was over? Are they satisfied with his deputing the house-surgeon, who though highly talented, was yet but a pupil, to superintend the treatment of the more serious cases, apparently without control? Are they satisfied with Sir Anthony for writing a certificate which was to have the immediate effect of expelling a patient from the hospital, when you have evidence that he made no inquiry into the man's case, but merely said, "let him be kept quiet"? I care not whether the case was delirium tremens or not, I say Sir Anthony Carlisle neglected his duty in not seeing the case more frequently; he neglected his duty in trusting too much to the house-surgeon; and he was guilty of gross carelessness in stating that a man was labouring under symptoms of insanity, when the man was perfectly quiet and rational. If you are satisfied that Sir Anthony should declare in his certificate that the patient had been under his professional care from the 22nd to the 28th of August, when, in fact, he had only been seen by him for a few minutes on on Saturday the 25th; if you are satisfied with this state of things, it behoves you to say so, I shall acquiesce; but I shall not cease to feel sorrow for the fate of this noble institution, and for the unfortunate patients who may seek its shelter. I shall certainly press my motion.

Mr. WHITE said,—I apprehend, that any

physician or surgeon of this hospital is authorised to say that a patient is under his care, when that patient is admitted in his week, and attended in his name by the subordinate officers of the charity. I have always used this license of language, and shall continue to do so. Sir Anthony Carlisle, in transferring the intermediate care of his patient to the house-surgeon, has done no more than is the custom of this and of every other hospital in the kingdom. If the principal surgeons of institutions were to perform every minute duty themselves, where would the means of improvement, by exercise of skill and judgment, be afforded to the rising generation of surgeons. Sir, this system of deputation is, within certain limits, indispensable. It is impossible that the surgeon's hands can execute everything that his head devises; there must be combination of effort as well as division of labour. But, Sir, it is an entire mistake to suppose that the care of the surgeons' patients in this hospital is surrendered to incompetent persons. Mr. Dasent, our house-surgeon, is in his second year of residence; he is a member of the College of Surgeons, and is, by acquirements qualified, as he is by his diploma authorised, to undertake the management of the highest patients in the country. I say, then, we are guilty of no offence in distributing our less responsible duties among the students of different standing at this hospital; it is necessary to the welfare of the patient; it is necessary to the improvement of the pupil. Sir Anthony Carlisle, then, was innocently ignorant of the state of his patient, Holmes, from Saturday till Tuesday, for he fulfilled all the duties which the laws of the institution require of him. When he saw Holmes on Tuesday there is little doubt that he had previously made up his mind as to the nature of the malady, and it was only natural and consistent with his former experience, to suppose the patient's tranquillity to be the transient effect of opium. It is a matter of poignant regret to me that this accusation should have been brought forward against Sir Anthony Carlisle,—an old and faithful servant of the charity, a friend to it when it needed friends, a man whose scientific labours have shed honour on the institution, and enlarged the bounds of knowledge. I feel some surprise that the accusers of Sir Anthony should have chosen so obscure a theme as delirium tremens for their *cheval de bataille;* a disease which is admitted on all hands to be with difficulty distinguishable from two analogous affections, mania and febrile delirium. I must confess to you that I believe cases frequently occur in which it is impossible for the medical attendant to decide whether the case be delirium or madness. I have had numerous cases of this description,—cases in which the delusion has been fixed, and in which the tremor never occurs or quickly disappears. It is only a few days since I was called in to a case of delirium in which the patient presented all the characteristics of mania. He was pallid; he sat on his bedside apparently calm, but he suddenly would start and exclaim, "there, don't you hear it in the wall!" This idea continually recurred to the patient's mind. Nothing short of an accurate examination of the previous life, and habits, and connections of the individual, can furnish a satisfactory diagnosis in these cases. It does appear monstrous to me to make a slight error of judgment, which might occur to any of us, the burthen of an overwhelming charge that is to make shipwreck of an old man's fortune and reputation! Good God! who can say that any particular paroxysm of delirium shall ever terminate? Whilst a patient suffers from delirium he is, for the time, insane, and no medical man can safely say whether it will terminate in death, in permanent insanity, or in convalescence. *I* would not undertake to prognosticate any one of these results in any case; and I am ready to place myself in the same boat with Sir Anthony, and to declare that I know not how to distinguish insanity from delirium tremens, at least in many forms of both diseases.

Mr. Cyril Page, Mr. Bicknell, Mr. Green, Dr. Roe, and Dr. Bright, having made a few colloquial remarks,

Mr. THOMSON spoke as follows:—I must express, Sir, my admiration of the hardihood of Mr. Cheere in venturing to bring forward an inquiry into the conduct of a medical officer in a committee in which fourteen members are medical men, and seven only not medical. What could induce Mr. Cheere to suppose that medical men could ever be brought to pronounce judgment on the conduct of a colleague, however flagrant? In my opinion no charge was ever more fully made out, than this charge of neglect against Sir Anthony Carlisle. If I stood in any other relation than that of colleague to Sir Anthony, I should take very different steps in this matter; but as it is, I think it my duty to state frankly to the Committee my opinion that the disease of Thomas Holmes was a clear instance of delirium tremens, and that the error of Sir Anthony, in taking it for insanity, was a gross mistake. I am not singular in this opinion, even among my colleagues; for Dr. Roe has just stated to me his belief that the case was an unambiguous one of delirium. This is also the impression of the house-surgeon, and even of the pupils. Sir Anthony was the only person who did not comprehend it. I have seen the progress of the case from beginning to end, and I do not stop at asserting that Sir Anthony did not pay proper attention to the case; he made no careful inquiry into its merits, but judging at a glance wrote the rash and unwar-

fantable certificate which was to jeopardise the life of an unoffending man. But it is in vain, Sir, to expect justice from a Board formed like the present. Medical men always prefer the *esprit du corps* before the broad principle of universal justice. This Committee will not pass a true verdict on this inquest. They will wait till harsher and more effectual measures shall secure the end proposed, justice to the patients and to the institution, by a proceeding more painful and severe.

Mr. THYNNE, the brother-in-law of Mr. Lynn, was of opinion that if the fourteen medical men would retire, and leave the matter in the hands of the seven non-medical, the latter would soon come to a decision on the subject, and decide in favour of Sir Anthony.

After some discussion amongst the supporters of Sir Anthony, of different shades, it was determined to meet Mr. Cheere's motion with a direct negative. That gentleman had no objection. He therefore read his resolution, but getting no one to second him, it fell to the ground.

Mr. THYNNE drew up the following form of motion, which was regularly supported and unanimously carried:—

"That the charges against Sir Anthony Carlisle, are not proven, and that this Board adjourn *sine die*."

DR. GRANVILLE ON COUNTER-IRRITATION.

To the Editor of THE LANCET.

SIR:—In my recent work on counter-irritation, I addressed the general reader more than my professional brethren, for reasons which are fully and openly stated in the preface, and which, I trust, will have their just weight when duly and impartially considered.

Having in the course of nine years experienced largely the benefit derived from a more effectual, as well as a more frequent and prompt use of counter-irritation, on the external parts of the body, in the treatment of many important diseases, it was natural that I should desire to propagate my conviction respecting that mode of treatment, among those who were likely to require it, namely, the public. But in order to enable the public generally to appreciate the value of a practice on which so little has been written in this country, and which, discarding the usual farrago of medicaments, professes to accomplish much by very simple means, it was, above all other things, important that a plain and popular exposition of all the principles and facts appertaining to the question of counter-irritation, should be laid before the world. This could not have been effected by any work written in the language of the medical schools; nor by one exclusively addressed to the profession; for had the work been thus written, it must have remained a sealed volume to the most interested parties. The contrary course, therefore, was adopted. On the other hand, I had also to inform the public, that there existed a new and more energetic combination of remedial agents, calculated to carry on and expedite the treatment of diseases by counter-irritation. But before I could so inform them, it became evidently necessary that a full and comprehensive review of the nature and effects of all the counter-irritating remedies hitherto known and usually employed, should be given. In executing both tasks, it is manifest that, whatever value I might have been disposed to set on the manner and fullness of their performance, as likely to be of some little service, even to medical readers, there could have been nothing, in reality, in my performance that could be called intrinsically important to them, save a more intimate knowledge of the new agents to be employed, which might be reserved for a future communication through a more appropriate channel of publication. I, therefore, thought it right, for the present, to confine myself to a popular view of the matter, in order, as I stated in my preface, to prepare the general reader for a more frequent use of counter-irritation in many diseases in which it had never, or seldom, been employed. By so doing it appeared to me that the interest of medical science would be best promoted; and with that view, and for the purpose of facilitating the adoption of a counter-irritating or external treatment of disease, I named and described at length the class, nature, and effects of the particular counter-irritating agents I had found to be endowed with greater efficacy than any hitherto employed possessed, without entering into any further medical detail. This information I supported by a body of evidence, which I hoped would be deemed equally suited for the perusal of the professional and non-professional reader.

Thus far I fulfilled a duty which every medical man owes society, to have gone beyond it would have been a dangerous experiment, that of rendering the public independent of the medical faculty; an experiment which no honest practitioner should ever attempt, inasmuch as it will always redound to the injury of the community. Having, therefore, named the agents I had employed to produce the striking effects which were illustrated by many cases brought forward on the occasion, I abstained from specifying the several pharmaceutical manipulations which properly belong to the profession, and with which the non-professional readers could have nothing to do, I contented myself, in fact, with simply stating, that "by merely regulating the several proportions of those

agents, according to the nature and intensity of the case we have to treat," all that I predicted would be obtained. In this manner I threw the responsibility of the active treatment into the hands of the medical attendant, who alone could determine, according to the exigency of the case under his care, the proportion to be employed, from what he must have read, in common with his patient, of the effect produced by the agents in question, in the several sections of my work devoted to that subject.

But although my impression was, that the medical attendant, with the data I had given him, is competent to determine the proportions for himself, I felt at the same time, that in order to carry out my intention of seeing the practice of counter-irritation in the treatment of diseases spread wide among the profession (so that their experience of the new mode I have suggested and recommended for that treatment might confirm the views I have adopted respecting it), I had another duty to perform. That duty consists in giving publicity, through a professional journal, to the proportions of the ingredients used in preparing the ammoniated or counter-irritating lotion, which I had myself, in the course of the last nine years, employed most frequently, either as a vesicant, or as simple counter-irritant, intended to arrest pain.

To several medical gentlemen resident in the country, who preferred being made acquainted at once with the proportions I myself employed, rather than try to ascertain them by their own experiments, I communicated the formula by letter; and among the most recent of those communications I may mention one to Dr. Colley, of Cheltenham, who applied for it about three weeks since. To others of the profession, resident in London, I offered (as I now offer to any respectable medical person) to show the mixing of the ingredients in their several proportions, and even to exhibit some of the physiological effects on the skin produced by them. The chemist, too, who, from the first, has prepared the lotion for the use my patients, was authorised by me, from the day of the publication of my work, to communicate the proportions to any of the faculty with whom he might be acquainted, and who might apply to him for that purpose. But these modes of propagating an useful invention in medicine are slow and imperfect, whereas that which a medical journal of high repute and extensive circulation offers, is the most effective and expeditious. I have, therefore, selected THE LANCET for that purpose, being convinced that, through its pages, my desire of putting my professional brethren in possession of the means to procure for themselves a remedial agent which, I confidently expect, will afford them satisfaction, will be accomplished, even to the remotest parts of the country.

After perusing the following formula, and having judged of the strength of the compound lotion resulting from it, by the description given in my work of the powerful physiological effects it produces, your readers, Sir, will agree with me that to have placed indiscriminately in the hands of the non-professional public such an agent would have been highly injudicious; nay, I would say dangerous, considering what we have been witness to within the last few years in regard to counter-irritation, employed by an unskilful non-professional individual, ignorant of the principles and practice with which he ventured to meddle; indeed, even to the medical profession, it will not be an act of superfluous caution to tell them, that in the discrimination of the cases which may require the use of an ammoniated counter-irritating lotion; in the mode of applying it; in the estimation of its physiological effects; and, lastly, in the manner of treating the parts after its application, they had best follow implicitly the directions given at full length in sections iv., vi., vii., viii., and ix. of my volume on counter-irritation, and which it would be impossible to repeat in the columns of any journal.

I shall now proceed to describe the mode of preparing the two sorts of ammoniated lotions, each of different degree of power, usually employed by myself in the cases I have published, and conclude with one or two comments in illustration of the process.

Each kind of lotion consists of three ingredients:—

1st. *The strongest liquor of ammonia*, A;
2nd. *Distilled spirit of rosemary*, B;
3rd. *Spirit of camphor*, C.

PRELIMINARY STEPS.

A.

Saturate a given quantity of distilled water, contained in a glass receiver surrounded by ice, with ammoniacal gas, obtained in the usual way, from a mixture of equal parts of hydrochlorate of ammonia and recently slaked lime, both reduced to a fine powder. The water may be made to take up nearly 800 times its bulk of ammoniated gas under the circumstances described; its specific gravity will then be about 872, and 100 parts of it will contain 33 parts of real ammonia according to Sir H. Davy's tables. This solution of ammonia will, therefore, be more than three times the strength of the *liquor ammoniæ* of the Pharmacopœia of London, 100 parts of which, at a specific gravity of 960, contains only 10 parts of real ammonia. I have, therefore, called mine "liquor ammoniæ fortissimus."

B.

Take two pounds of the tips or small leaves of fresh rosemary, and eight pints of

alcohol; leave the whole in infusion for 24 hours in a well covered vessel, and after adding a sufficient quantity of water as will just prevent the empyreumatic smell, distill over *seven* pints. The Pharmacopœia of London directs the essential oil of rosemary to be distilled instead with rectified spirit. Such a preparation I found unsuited for my purpose.

C.

To four ounces of pure camphor add two pints of alcohol, so as to dissolve the camphor, which solution should be filtered. The present *tincture of camphor* of the Pharmacopœia of London, contains one ounce more of that substance, and does not harmonise so well with my two other ingredients as the weaker preparation.

The three ingredients, thus prepared, every medical man should keep always ready at hand in well-stoppered glass bottles, so as to be able to make, extemporaneously, a counter-irritating lotion of any requisite strength, according to the nature of the case requiring that application on extraordinary occasions. But for the ordinary purposes detailed in my work it will be better to keep both a milder and a stronger ammoniated lotion ready prepared for use.

The milder Ammoniated Lotion.

Assuming the quantity of lotion desired to be divided into *eight* parts, then the proportions of the ingredients will stand thus:

A—four-eighths;
R—three-eighths;
C—one-eighth.

The stronger Ammoniated Lotion.

If the quantity desired be also divided into eight parts, then the proportions of the ingredients run as follow:—

A—five-eighths;
R—two-eighths;
C—one-eighth.

Although the changes of proportion here may be deemed trifling, yet the strength of the lotion is such that I never employ it except in cases of apoplexy, and for the purpose of cauterisation.

Directions in mixing the Ingredients.

A and R are gradually mixed together. The mixture becomes opalescent and somewhat turbid, and a peculiar highly-agreeable ethereal smell is given out, different from the individual odour of either ingredient, although the extreme pungency of the ammonia be still discernible. I have strong reasons to believe that, at this point of the operation, some particular change takes place, imparts to the mixture of the two ingredients some of its valuable peculiarities as a counter-irritant described in my work; but what that change is, it is not my business to enter upon in this place: suffice it to say, that in a great number of experiments made with the ingredients separately (for each of them acts as a counter-irritant on the skin) and with them combined, the effects were uniformly different; those in the former case being found unequal to the production of those complete results which I trust I have justly promised to the profession. Ammonia alone (however strong) will not give rise to the effects I have described, though it has often stopped internal pain and produced *small little blisters*; but never has it succeeded in almost immediately producing a full vesication, as I have seldom failed to produce with the two ingredients mixed together, particularly after the third ingredient has been added.

Before, however, that third ingredient is so added, it is desirable to clear the previous mixture, by the addition of a small quantity of alcohol, and to set the whole in a cool place. All the various precautions here mentioned may, upon an emergency, be dispensed with, when an immediate action is required, either to arrest pain or relieve deep-seated inflammation. But for the more delicate uses, particularly for instantaneous vesication, the preparations should be obtained in the manner I have specified.

The lotion must always be kept in bottles with a glass stopper; and their whole virtue depends on the accurate distillation and preparation of the ingredients, as well as on the careful admixture of the latter. The species of ethereal principle formed during the admixture remains present in the lotion, but it is apt to vanish if the bottle be frequently opened, and then much of the peculiar effect of the counter-irritation is impaired. It is one of the many recommendations of these powerful preparations, that their effluvia, besides being agreeable, are of precisely that nature which is most likely to revive and benefit the patients labouring under diseases that require the application of counter-irritants. The compound camphor liniment is the only known combination of ingredients nearly similar to the ammoniated lotion just described. But the profession is well aware that the liniment will not produce, and never has produced, the effects I have predicated.

Among those effects, one of the most surprising is that of giving rise, in a space of time varying only between three and ten minutes, and in almost every instance (if such a result be the desired object), to as ample and full a vesication as can be expected in as many hours from the best Spanish flies. This is a result which I am not aware has been obtained before in so short a time, except by boiling water (a remedy not quite so pleasant as the odour of ammonia); and on it, therefore, as well as upon its importance in the treatment of many serious disorders, I do take my stand, as also upon that of arresting nervous and

muscular pain, almost immediately, provided it does not depend on structural disease. Requesting you to take a note of these facts and assertions, and of their date, I remain, your obedient servant,

A. B. GRANVILLE, M.D.

Grafton-street, Oct. 22, 1838.

TARTAR EMETIC IN SPASMODIC CHOLERA.

To the Editor of THE LANCET.

SIR:—From frequent opportunities of witnessing the beneficial effects of tartar emetic in some forms of delirium tremens, more especially those in which vomiting and a relaxed state of the bowels exist, I was induced to try its influence in spasmodic cholera: the result, in several cases, has been such as to render it worthy of more extensive trial. Those who are in the habit of relying upon calomel and opium as their sheet-anchor in this disease, will have, in the majority of cases, to lament the obstinacy of the vomiting by which, almost as soon as given, every dose will be rejected, and thus the disease go on to a fatal termination; but the speedy manner in which the most severe action of the stomach is arrested and subdued by tartrate of antimony, constitutes the the chief peculiarities in its use. When in London, during the epidemic of 1832, I had the most extensive opportunities of trying calomel and opium, in large as well as small doses, both alone and combined with each other; but with it, as with other plans then practised, the results in the greater number of cases were highly unsatisfactory. Since that period the occurrence of the disease has not been so frequent as to render my experience extensive in the remedy I now propose; I however, beg to subjoin the two following cases:—

CASE 1.—J. H., æt. 56, of inactive habits, had, for a few days, complained of slight twitching pain in the bowels, with shivering and nausea. On the 29th October, 1837, at 9 A.M., I was called to visit him; he had then been vomiting every 10 or 15 minutes for some hours; frequent dejections, like rice and water; severe muscular spasms, in upper and lower extremities; loss of voice; skin livid and contracted; cold, clammy, perspiration of the face, chest, and hands; no pulse perceptible at the wrist; slight epigastric tenderness; pulsation of abdominal aorta labouring; most anxious expression of the countenance, and, to all appearance, as if the vital powers were rapidly sinking; tongue cold and moist. Ordered to have frictions to the body, mustard plasters to the legs and feet, a large blister over the epigastrium.

Tartarised antimony, viii. grains;
Calomel, xx. grains;
Opium, xii. grains. Eight pills; one every hour.
Carbonate of soda, ii. drachms;
Aromat. confection, i. drachm;
Peppermint water, viii. ounces. Two teaspoonfuls every second hour.

2. P.M. Has not vomited for more than an hour; surface more natural; pulse perceptible, but feeble; cramps less frequent; stools as frequent as before; has not evacuated the bladder since yesterday. Continue remedies.

8. P.M. Has vomited twice since last visit, in both instances after taking the mixture; appearance improved; complains of no pain, except from the mustard plasters having produced vesications; reaction becoming established; pulse 110; has had four stools, the last having a greenish hue. The catheter was introduced, and about half a pint of urine removed. Discontinue mixture, and omit the pill to every four hours. To have beef-tea, &c., in small quantities frequently.

30, 9 A.M. Has slept frequently during the night; says he feels very low, but free from pain; in every respect better. Omit the pills. From this time he continued to improve rapidly, although the retention of urine remained for several days.

CASE 2.—H. W., æt. 40, having a child at breast 14 months' old, has been frequently the subject of disease; bowels had been somewhat irregular for three or four days, but on Thursday, the 20th September, 1838, had vomited and been purged frequently. About 2 o'clock A.M., on Friday, she became considerably worse, and at 8, A.M., I was sent for. She was then in a most severe state of collapse; the whole surface of the body was livid; the features contracted, and indicative of extreme suffering; the tongue blanched and cold; a clammy perspiration about the hands, which, together with the breath, has the sickly odour so peculiar to this disease; cramps in both upper and lower extremities very severe; the voice was gone to a mere whisper, except at such times as the spasms were almost intolerable, when the articulation broke forth in a painful whine. She was vomiting every quarter of an hour a transparent fluid of a greenish hue, and had rice-water dejections every half-hour; no urine had been evacuated the afternoon of previous day. Pulse 100, almost imperceptible. A large blister to be applied over the epigastrium; turpentine embrocation, with friction to the extremities.

Tartarised antimony, vi. grains;
Calomel, xxiv. grains;
Opium, viii. grains. Eight pills, one every hour.
Liquor of acet. of ammonia, 1½ ounces;
Aromat. spirit of ammonia, i. drachm;
Spirit of nitrous ether, ii. drachms. Two spoonfuls every second hour.

2. P.M. She vomited after the first pill and twice since, in both instances after swallowing the mixture (ordered to be discontinued); has had three stools of a like character to the last; appearance somewhat better; pulse 120, very feeble; cramps not so frequent. Continue pills. To have small quantities of sago or arrow-root.

10. P.M. Pulse as quick as before, but stronger; has only vomited once since last visit; feels very much exhausted; no tenderness or distension over the hypogastrium; has parted with two dejections, but no urine; temperature of the surface nearly natural. Omit the pill to every three hours.

22, 10. A.M. Has slept a little during the night; no pain, no vomiting; has had three stools, the last about a pint of nearly transparent serum, with a precipitation like boiled rice, and precisely the colour of the acetas cupri; countenance improved. Discontinue pills. To have beef-tea, &c.

9. P.M. Having passed no urine, I introduced the catheter and drew off about eight ounces of turbid urine, of a very pungent odour; pulse continues quick, but regular.

Carbon. of soda, ii. drachms;
Hydrocyan. acid, viii. drops;
Acet. of morph. ½ grain;
Peppermint water, viii. oz. Two spoonsful every third hour.

23. Has had three or four evacuations, tinged with bile; pulse 110; very thirsty; in other respects better. She continued to improve slowly but steadily; the inability to expel the contents of the bladder required the use of the catheter for a day or two, and on Oct. 2nd she was quite convalescent.

Dr. Graves of Dublin, in explaining the *modus operandi* of tart. emet. in delirium tremens, attributes to it narcotic effects, and in looking over vol. 2, 1835-6, of THE LANCET, for his remarks, I find a communication from Mr. Wm. Greenwood, of Kendal (which I had not previously seen), who details a case illustrative of such effects in a very striking degree. Whether, with those gentlemen, we should consider it to be a narcotic, or, with Tomassini and others, a counter-stimulant, it is not my intention here to inquire; but having laid these *facts* before the profession, I leave it to the reflection of all unprejudiced practitioners, either to adopt or reject a remedy which, so far as I have observed, possesses considerable power in arresting the symptoms of an hitherto almost uncontrolable disease. I am, Sir, your very obedient servant,

G. FEARNLEY, M.D., M.R.C.S.L. & E.

Church-street, Dewsbury,
Oct. 12, 1838.

THE LANCET.

London, Saturday, October 27, 1838.

IF the English lunatic asylums have not been forgotten during the last twelve years they have been greatly neglected. Since the Parliamentary Committees sat in 1807, 1815, 1816, and 1826, notwithstanding the disclosures then made, general attention has never been fairly directed to these institutions, or to the inefficiency of the public boards of inspection and regulation. This is greatly to be deplored; no branch of medical police is of greater importance to the public, or more interesting to the medical practitioner, who may be called upon to give advice, or evidence, on any day in the year, and should always be prepared to do his duty. We shall, therefore, rapidly examine the state, the effects, the abuses, the laws, and the public management, of asylums for the insane.

The subject is uninviting. Intellect is so much the essence of human nature, that men deprived of reason seem to forfeit all claim to fraternal sympathy. Sensitive persons turn away with instinctive abhorrence from the manifestation of headlong passions, exaggerated pretensions, weakness, wickedness, folly,—in a word, from INSANITY, in all its multitudinous forms: for it is, as it were, the corruption of the mind, passing from the Divine image to apathy and death. It was beautiful in its workings—that brain—and will be ashes, but madness *is* horrible. This consideration enables us to understand the treatment which lunatics receive among savages, and the wretched sufferings to which they have been doomed in prisons and asylums in England. The life is *extinguished* by the savage,—it is allowed to *waste away*, in isolation, and dens, and chains, in England. And what has civilisation to object to these practices? What, it may be asked, after PLATO, has society to gain by the protracted existence of lunatics? What, in England, too, where population,

according to some philosophers, is increasing so much faster than subsistence? Much. It is a law of Nature that every man should be liable to innumerable diseases,—secure from none. No one can look forward with certainty to a constantly serene course. The heart that beats well to-night may fail on the morrow; the subtle brain, playing in all its might, and, throwing off thick-coming thoughts, may in a day be cast into irreparable disorder. You stand secure, calm, believing steadfastly in your fate; but know you all the secret cells of madness? Have you good security against exposure to its causes? Your passions, may they not be overstretched, your enthusiasm exalted? The extraordinary circumstances in which you are placed, can they not lead to distraction? And cannot that fortune,—the riches, friends, household stuff of happiness, on which you count in all your calculations,—desert you in the hour of trial? Oh! there are many ways to madness.

What, then, sustains the provident citizen under the diseases which hang like threatening clouds over his life, and the lives of his dearest friends? Is it not the consciousness that if disease should come, every means will be employed calculated to restore the body to health,—that if the affliction prove lasting, it will be outlived by tenderness, and that at last the head will be let fall gently upon the breast of the parent earth? But, with the certainty of neglect in sickness, of unkindness, and of cruel torture, staring him in the face, how could any man be happy? Do us the favour, considerate reader (only for an instant), to fancy yourself mad, and follow yourself to a lunatic asylum, where the proprietors are paid in *proportion to the duration of your disease;* watch your hands manacled, and listen to the brutal keeper's voice. See your own mother in the situation of Mary Ann Strong,—or imagine the feelings of a husband, when his wife, with a heart dilated and a brain inflamed, writhes under the insulting scourge, stands naked in the open yard, and retreats, wet and shuddering, to the straw-house! Hear the poor lunatic herself—true to the woman's heart—and conscious rather of the shame than the physical suffering, addressing her relative,—"*I was naked* but a short time before I was brought to you;" and, pointing to her foot, "*I wear a chain.*" And do not forget that there are thousands of lunatics exposed to similar cruelties all over the country, and that the whole system is governed by ill-constructed Commissions and Acts of Parliament. On the ground, then, that all mankind are liable to the dreadful disease; that, by proper measures, great numbers now chained in madhouses may be restored to reason; and, finally, that the protection of the helpless lunatic is felt to be an imperative duty by every benevolent person, all natural repugnance to the subject must be overcome, and all its dry, revolting details must be carefully examined.

The inquiry is of greater extent than persons who attend to it for the first time, imagine. From a return made to the House of Commons (1836), it appears that the number of pauper lunatics was as follows:—*Paupers* confined in asylums built under the provision of the 9th George IV., c. 40,—as *lunatics*, males, 1260, females, 1350; *idiots*, males, 90, females, 801;—confined in private lunatic asylums, as *lunatics*, males, 659, females, 744; *idiots*, males, 35, females, 53;—under the care of parish officers, as indoor or outdoor paupers, *lunatics*, males, 915, females, 1474; *idiots*, males, 3247, females, 3760. Total, lunatics, 6402; idiots, 7265; lunatics and idiots, 13,667. Lunatic paupers and idiots alone form 0.00098 of the entire population, or 1 in 1024. In 436 Unions, there were 5259 idiots (2602 from birth), and 3841 lunatics, of whom 2601 were deemed incurable. Of 1000 not more than 286 were congenital idiots; while the entire class of idiots amounted to double the number, or 578; 422 were lunatics, of whom 287 in the 1000 were considered incurable, 135 curable. From the last Parliamentary returns (1831), it appears that there were about 108

licensed lunatic asylums in England—70 in the country, 38 in the metropolitan district; and that the houses contained 4495 insane patients. If we assume that the number of pauper lunatics, in licensed houses, was 1491, as in 1836, it will leave 3004 private patients to be added to the 13,567 before enumerated. As the lunatics in private custody are not counted, and great numbers are known not to be enumerated, we cannot set down the insane persons in England at much less than 20,000. The majority of the insane are naturally paupers, as the maintenance of a helpless lunatic by poor people is absolutely impossible; but the rich are not exempt from insanity; and it was ascertained in 1833, that the clear annual income of 399 lunatics, confined under the Crown alone, amounted to nearly 305,966*l.* The incomes of 351 amounted to 269,158*l.*; the incomes of 48 were not at the time ascertained. The average annual income of each lunatic in the care of the Lord-keeper, was 766*l.* 16*s.*, and the entire property of these, if the clear revenue be 4 per cent. on its value, must amount to 7,649,000*l.* To this must be added certainly not a smaller sum for the 3000 private patients under confinement in licensed houses. The property belonging to the insane in England must amount to 14 *millions sterling*; and the clear annual revenue of their estates and properties to somewhere about 600,000*l.* a year. Whether, then, the numbers be regarded, or the immense wealth taken from lunatics and managed by Government, or by alien hands, our view of the importance of the inquiry is not diminished. Nor ought the interests of the proprietors of lunatic asylums to be overlooked, many of whom are highly respectable persons. Some have sunk large sums in their establishments; Dr. Fox, for instance, stated before the Committee of the House of Lords, that he had expended 35,000*l.* upon Brislington-House, near Bristol.

If the present Commissioners of Lunacy, and the public inspection, were efficiently carried on, what important facts might not be elicited! Innumerable observations, carefully made and recorded, would dispel the obscurity that hangs over the laws of this disease; and in the end lead to its extirpation, or to infinitely more successful modes of treatment than are at present known. It is impossible to advance a step in the inquiry without seeing, on every side, facts inviting us to scientific investigation. We shall point out a few of these; and the fault must be laid upon the gentlemen who are paid a guinea an hour for their services, if these facts be not numerous; for it is melancholy to reflect that although thousands of pounds have been expended upon Inspectors, and Visitors, and Commissioners, those gentlemen have never published a fact of the least importance to science. Science has been treated with as much neglect by them as the unfortunate lunatics.

It is difficult to determine the real causes of insanity; but this has been done to a certain extent at different places. An example will show that if the inquiry were extensively pursued, it would lead to interesting results. At the State Lunatic Asylum, Worcester, Massachusetts, it was found that of 678 patients admitted, 210 had insane ancestors or kindred, thus showing to what an extent the malady is hereditary.*

The determination of the proportion of recoveries is a vital question; for unless the general probability of recovery be known, how can the different asylums and different modes of treatment be satisfactorily judged? We give the following as the result of an extensive series of facts collected at the English County Asylums, Salpetrière, Bicêtre, St. Luke's, Bethlem, Charenton, the Senavra, Milan, and other asylums.†

Cases treated, 49,711; recovered, 20,890; Recoveries per cent., 42.2.

This is the general average; to show what differences and contrasts exist, the reco-

* Annual Report of the Trustees of the State Lunatic Asylum at Worcester (America), 1837.

† For the separate facts, and the authorities, see "Statistics of English Lunatic Asylums." Sherwood.

veries at Hanwell—the Middlesex County Asylum—may be placed in juxta-position with the recoveries in the other county establishments.

	Cases treated.	Recovered.	Recoveries per cent.
Hanwell	1183	222	19
County Asylums, England	4294	1977	46
District Asylums, Ireland	1589	722	45

In one case 46 per cent. recover their reason; in another only 19 per cent. are restored. Do not the causes of these discrepancies demand investigation?

The influence of age on recovery is shown by the following results deduced from observations made at Bethlem, 1784-94, by Dr. Haslam.

Ages.	Discharged. Cured.	Incurable.	Cured per 100. Fact.	Theory.
10—20....	78	15	69.	—
20—30....	200	288	43.	42.
30—40....	180	347	34.	32.
40—50....	87	275	24.	24.
50—60....	25	18	17.	18.
60—79....	4	27	13.	14.

Here are two remarkable general facts, that the rate of recovery decreases with age, and that it decreases very uniformly 33 per cent. every 10 years. How much is it not to be regretted that the Commissioners have never laid before the profession important scientific facts of this description?

The tendency to recovery increases during the first nine months; it then declines rapidly, as is plainly shown by the following facts, treated at Worcester.* It is to be regretted that the cured were not separated from the curable in the table.

Years' Duration.	Total Cases.	Cured or Curable.
0— 1.....	232.....	210
1— 2.....	94.....	63
2— 5.....	109.....	36
5—10.....	76.....	9
10—15.....	56.....	3
15—20.....	24.....	1
20— 0.....	17.....	0

M. Esquirol has given a table leading to the same inferences, that the tendency to recovery increases, and then decreases, according to a regular law, in the same manner as small-pox and cholera.

* Report, 1837.

The *duration of the cases* is always an important element. When it is stated that a patient has been cured, the time in which the cure was effected should be specified. We have often read of miraculous cures, and been surprised at the end to find that the cure of a very simple disease has been going on for several months, nay, years. It is a great object in insanity to shorten the duration of the disease; and the Commissioners of Lunacy, to determine the relative efficiency of asylums, should possess positive data, that might enable them to deduce the ordinary duration of attacks at different ages and in different circumstances. But they have attended to none of these things; patients are usually detained in the hospitals one year and three-quarters; in some hospitals for the insane they are detained three and four years:—

	Duration of Treatment in years.
Stafford Asylum	1.47
Lincoln	1.72
Gloucester	1.72
W. Riding Yorkshire.....	3.16
Hanwell	4.11

The difference in the duration of the treatment makes all the difference in the world in the expense of the pauper and the private patient.

We shall return to the consideration of this important subject.

In another part of this Journal will be found a full Report of the proceedings of the House-Committee of the Westminster Hospital. We shall, probably, offer a few remarks upon the result next week.

The Naturalist's Library. Publishing in Vols. Vol. VII. British Quadrupeds. London: Highley; Edinburgh: Lizars, 1838. Numerous coloured plates.

The non-professional subscribers to this interesting work must be imbued with the highest estimation of the medical profession, for scarcely can there be opened any volume of the series without disclosing evidence of the debt which natural science owes to men who have been educated for the healing art. In a former notice we placed in prominent

view the fact that even Bruce, the celebrated traveller, was wholly indebted for his opportunities as a traveller, to instruction which he had obtained in medicine. The facts formed a striking and valuable tribute to the highest of the arts. In perusing the history of natural science, the great majority of its best contributors appear to have been disciples of that art. Each volume of the "Naturalist's Library" is prefaced by a memoir of some distinguished discoverer, chosen with no partiality but for the greatest among his class, and as one result we find that without many exceptions the biographies are those of disciples of medicine. It is true that in some instances the philosopher has ultimately retreated from the paths of the one science to the more attractive field of another; but the inclination, the education, and the earlier pursuits in life have belonged to medicine, and the habits engendered by a medical education, and its fostering care of the mind and intellect during youth, have been causes of the subsequent career and great discoveries of the man.

At the head of the volume just issued, stands a memoir of Ulysses Aldrovandi, in whom we have another illustration of our remark,—a medical practitioner who, with an energy and perseverance which knew no bounds, laboured so successfully in his department as a naturalist, that he was designated by his contemporaries as "the modern Pliny." Haller calls him the most distinguished naturalist of his time, and Bayle described him as the most inquisitive naturalist in the world. Aldrovandi commenced his career three centuries ago,—long enough since for time to obscure the finer traits of his character, and many interesting incidents of his life. The lapses in his history are, indeed, so numerous as to be wholly inexplicable. But, fortunately, he has, with his own hand, while rearing the great memorial of his works, left to us some materials for a scanty sketch of his life. He was born in Bologna in, or soon after, 1520, and died in 1605, aged more than 80 years. His extraction was noble. His education was completed at Padua. Having travelled to Rome to vindicate himself from the charge of free-thinking (when an octogenarian, by-the-by, he displayed himself in a title-page, kneeling at the foot of Pope Clement, surrounded by cardinals), he was smitten with antiquity, and published his first work, on ancient statuary. Probably his acquaintance at this time with the celebrated Rondolet, and his "History of Fishes," decided his future pursuits. He took a degree in physic at Bologna in 1553, and in the next year became the professor of natural history and botany. He then laboriously worked as a physician, and also as a teacher of medicine in the University, bearing away the palm of distinction as a professor, "not only" (says Miræus, by a licence in eulogy which is of the poetic kind), "from those who lived before him, but from those, also, who should succeed him." For natural history he entertained an unbounded love. He esteemed "nothing so much," he writes, "as this noble study, thus directing his views as to his earthly *summum bonum*. To it," he adds, "I devoted every word and action, in fact, my whole labour. I was like the mariner directing his course by the polar star, not forgetting the precept of the Stoics, that every man should have some fixed goal unceasingly before him. I felt that I must exert every nerve with the greatest ardour in the pursuit of learning. Laying aside, therefore, all other business and cares, I devoted myself entirely to study, believing that nothing was more worthy of men than to seek to know those matters with which the Almighty alone seems to be familiar."

Such men make the great characters in human history.

The money which he expended in his inquiries was very great. He used to visit the country for months in each summer and autumn, not, like others, for relaxation, but to induce the country people, by gifts, to bring him insects, from the fields, from underground, from the air, the rivers, the ponds, with such accounts as could be had of their names, habits, and localities. In these excursions he was always attended by draughtsmen and amanuenses, to make sketches, and take notes at his dictation. At home he retained the services of one celebrated painter for thirty years, at a salary of 200 golden crowns. He engaged the celebrated Bennini to superintend his engravings; the famous Ligotti to delineate his birds in the most exquisite manner possible; and a host of first-rate wood-cutters to carve his smaller subjects. As for the collection of paintings which he accumulated to illustrate his observations, it was so vast and admirable that Napoleon, even in his day, seized on them as a prize for the Museum of the *Jardin des Plantes*, where they were deposited until reclaimed, in 1814, by the citizens of Bologna. In this way he collected a vast museum, and produced invaluable massy folios of discovery. Natural history had not, in his time, divided itself into distinct fields, possessing votaries who could confine themselves to favourite departments. Hence the extent of inquiry opened and followed by Aldrovandi, was so vast that any mind and energies other than

such as his own would have been daunted at the task. For he almost seems to have aimed at investigating personally *the whole* of Nature's works. Ardour and enthusiasm so unwonted and burning, however, were at least likely to be contagious. Accordingly, numerous devoted admirers and pupils aspired to aid, if they could not equal, his labours and acquirements.

Amongst the results of his exertions, was the great Botanic Garden of Bologna, which proved a powerful stimulus among the philosophers of Europe; the enormous Museum already mentioned; and his many-volume "Opera," on which he occupied so many artists. He finally bequeathed his enormous store of scientific treasures to the Senate of Bologna, who made them the foundation of the great public museum of that city, many of his preparations, nearly 300 years old, remaining to this day. In the Public Library of his native city is still to be seen the prodigious mass of valuable manuscripts which he left. How various were the inquiries which ended in these, may be judged from the circumstance that there is a letter extant in which he treats of *Egyptian buildings*, which probably became accidental objects of his examination during his researches as a traveller. Indeed, his writings prove that he had managed to become intimately acquainted with the most recondite mythology of the country of the pyramids. When abroad he was equally a discoverer of articles for the materia medica, and an investigator of comparative anatomy, botany, and mineralogy,—not as a man labouring uncouthly in a barbarous age, but, as his present biographer remarks, rendering himself "a pattern to the most accomplished of modern zoologists,—at once the Hunter, the Humboldt, and the Cuvier of his day."

Respecting his private history nothing appears, excepting that he was once rich and became poor; and that he was married. Of his wife we can only say that when a widow she spoke highly of his virtues, though she could have had but little opportunity to study them. This is in his favour, for absence is a rock on which the bark of matrimony often splits.

The great object of Aldrovandi's life was his "Opera" on Natural History, the latter parts of which were not printed until he was dead. On this work he was employed for fifty years. It was published with a lavish expenditure, in the most sumptuous style. He exhausted on its contents the whole of his fortune and receipts as a physician, becoming at last an object for pecuniary aid; but so little is known of his domestic life, that up to this time it has been disputed whether he died, as Bayle declares, "a neglected pauper in a hospital," whom Nature herself so wronged at last as to blind him, or whether the Senate of Bologna became an honourable benefactor to one who had bequeathed to the public his wonderful museum and manuscripts, and whose works they were induced, after his death, to complete for publication at a very great cost. Cuvier, who wrote a notice of him, feared for the one event, while he hoped the other. But original documents supply negative evidence against his destitution, and his widow, in a dedication of his sixth volume to the Senate of Bologna, speaks of her husband in terms that indirectly oppose the melancholy view of this question.

He began to prepare materials for this work from "a feeling," as he himself says, "that he was specially endowed with the talent of investigating the deep secrets of Nature," and he ultimately reached a span of life which enabled him to leave to posterity some grateful memorial of "the blessings he had received from the Divine Majesty." Ever an enemy to idleness, he says, "I applied myself to these hidden subjects by day and night, without much regard to health, without caring for the colds of winter, or the burning heats of summer," and grudging no bodily pain or devotion of time. In arranging his work he placed first the *birds*, "whose very difficulty of study provoked prior investigation," and because of the character of their wings (which are "regular and not membranaceous") as compared with those of insects. To *quadrupeds* he gave the second place, notwithstanding they seemed to claim the first, "from their assisting man in his labours, and chiefly supplying him wirh food." The third place he assigned to *fishes*. The fourth to the *white-blooded animals*. This order would not now be very scientific, but Aldrovandi's task was rather to accumulate facts than create systems. Then he entered upon the vegetable and fossil worlds; and in the midst of, and throughout all, he presented very many things relating to public and private life, regulation of manners, ethics, rural and domestic economy, politics, military affairs, and the preservation of health, together with much information even for the linguist, whom he supplied with numerous synonyms in Greek, Latin, Hebrew, Arabic, Italian, and almost every other known language! Here was a plan to fulfil! But he promised its execution in his preface, and, marvellous to tell, he accomplished it "to the very letter, and in a way that renders it a lasting monument of his genius." The division on ornithology alone extended to upwards of 2000 folio pages, illustrated in a most beautiful manner by engravers. He was 77 years old when that volume was issued. The quadrupeds came forth in the next year,—a work of equal dimensions and merit. The treatises on insects and on serpents appeared in two years more, and, soon after, that on birds. Then he died, and in a year after that (1606) came a volume on the white-blooded animals. Other folios, ultimately

completing his "Opera" in thirteen volumes, appeared subsequently, under the direction of his colleagues and pupils, illustrating the fishes, the quadrupeds, monsters, fossils, and, finally, dendrology. He also wrote a work on natural theology. In fact, his works were as ponderous as our great modern encyclopœdias. How easily and successfully could a man of such a stamp step aside to write on ancient statuary and Egyptian architecture!

In his career he was patronised and aided by Popes Clement VIII. and Sixtus, Cardinal Montalto, and the Senate of Bologna. The aid of these munificent supporters of learning was timely and efficient.

Not only may medicine be proud to rank such a man amongst its disciples, but the public itself must proudly recognise in the profession that produces to science such sons, claims upon its respect and gratitude of the highest and most enduring kind.

Of the work, from 60 pages of which we have immediately derived the facts presented in this sketch, we can only repeat our remark that it forms the best possible introduction to comparative anatomy, inasmuch as a knowledge of the external forms of animals should precede that of the internal and anatomical.

REGULATIONS OF THE COLLEGE OF SURGEONS.

Dr. Howison, lecturer on botany at Edinburgh, has forwarded to us the following remarks, in which he urges the College to alter the paragraph,—

"One course of materia medica with medical botany, during six months,"—into

One course of materia medica, three months; botany, three months, omitting the word medical, as is the case in the regulations at present in force.

The following are the reasons upon which Dr. Howison grounds his request:—

First. How is a student to be taught medical botany if he be entirely ignorant of general botany, which more than three-fourths are? What is he to benefit by being told, in describing conium maculatum, or hemlock, that it belongs to the Umbelliferæ of Jussieu, and to Pentandria Digynia of Linneus, if he is ignorant of the nature of the calyx, corolla, &c., of the Linnean and Jussieuan arrangement? Any individual acquainted with general botany must be aware that it is a science so extensive as not to be acquired in a shorter period than three months, with the greatest industry, opportunity, and attention; and that medical botany is merely a more advanced branch detached from it as a whole. In fact, medical botany means nothing precise. The individuals who introduced the regulations now complained of, never were enthusiastic or proper botanists, and their having done so, in such a careless manner, is a sufficient proof. Every medical man ought to know general and medical botany, as an interesting department of his profession; but he can only acquire that knowledge by attending, diligently and attentively, one or more courses of general botany, properly conducted, of three months' duration, with an ample supply of recent plants. The Army and Navy Medical Boards deserve great credit for, and have given general satisfaction by, their regulations connected with this subject. The Navy requires six months of general botany; the Army four months. The Apothecaries' Company of London are entitled to the same merit. Might not the College of Surgeons of London, as far as surgery is concerned, with equal credit to themselves, have legislated that the student should attend operative surgery, without any previous knowledge of general surgery? Or, as far as anatomy is concerned, that he must acquire a knowledge of one quarter of the body, whilst the other three are cast aside?

Second. Botany and materia medica ought to form separate and distinct courses. They are never properly taught together. Botany cannot be taught during the winter season, the time at which the greatest justice can be done to materia medica.

Third. Combining materia medica and botany in one course of six months, which teachers in Scotland will now be compelled to do, in consequence of the regulation of the Royal College of Surgeons in London, above complained of, to make their ticket qualify, is most improper. The botany will be imperfectly taught, if taught at all; and men are forced to come forward,—namely, teachers of materia medica,—to make it known to others, who have never given it their practical attention, and who are ignorant of it themselves. Botany is not the superficial study which men ignorant of its minutia suppose. It is only to be acquired by industry, labour, the toil of summers in the fields, aided by a natural genius or attachment for its study. If Linneus, or James Smith, and many others, whose lives were totally occupied in its pursuit, had been asked how botany ought to be taught, their answer would never have been, in conjunction with materia medica, or by individuals forced into its service.

My recommendation is,—That every medical student should attend one three months' course of botany, at least, during the summer, with excursions; and after that, during the winter, one course of materia medica, with medical botany and dietetics, of six months' duration; and that the same individual (of course a medical practitioner, as best qualified) ought to be permitted to teach both.

In teaching materia medica and botany together, the student ignorant of general

botany would not be benefitted in the slightest degree, and confusion would be made of the whole. Were the Royal College of Surgeons of London to make the change as above recommended, in their regulations, as the Apothecaries' Company several years ago did, there can be no doubt that, like them, they would give satisfaction to the public, to every liberal-minded man, and they would advance the respectability of the surgical profession throughout England.

I may add, have not the Royal College of Surgeons of London, in enforcing the regulation now complained of, broken in upon long-established usages, and set at defiance common sense? Their having done so must have proceeded from inadvertency, and it is to be hoped they will alter it. Were other public bodies to follow their example, insisting upon a similar absurd regulation, what would become of the chairs of general botany in universities and other schools? If botany be not encouraged and protected, being an expensive science to teach, and requiring much time to provide and select the wild plants of the field, because from them only can it be taught, and not from the cultivated plants of the botanic gardens, as the world generally supposes, it will be given up by the teachers at private schools; or it will be imperfectly taught without fresh specimens; and the student will be deprived of herborisings, a source of health, pleasure, and instruction, during the long days of summer, when every other study becomes unbearable. Teachers of botany ought invariably to accompany their pupils in excursions into the surrounding country, to point out to them the habitat of plants. Would not a course of herborisings, or what might be denominated practical botany, be of equal service to the medical man, as a course of practical chemistry, or one of practical pharmacy? It would be far more advantageous to his bodily health.

A circumstance not uncommon in oriental countries, would have proved fatal to a large detachment of the British Army in the Burmese war, had it not been prevented by the knowledge and activity of Dr. Wallock. In one tank of water, destined for the use of the Army during a halt in pursuit of the retreating Burmese, the water had been poisoned by the aconitum ferox, bruised and thrown into the tank by the enemy before evacuating the place.

Nearly twenty years spent in teaching materia medica, botany, with botanical excursions, in Edinburgh, I trust entitle, and I may add compel, me to make these observations for the benefit of the medical profession. The College of Surgeons of London may have made the regulation now alluded to, unaware of its imperfections; and I hope the remarks now made may induce them to adopt the suggestions which I have thrown out.

PRESERVATIVE QUALITY OF TANNIN.

To the Editor of THE LANCET.

SIR:—Believing that the following may prove useful to anatomists, and other persons who are concerned in the preservation of animal substances, may I request its insertion in your widely-spread periodical, THE LANCET:—

A short time since, when examining my collection of fishes in spirit, I found that a few of my rarest specimens, which had been taken out for the purpose of making drawings from them, and had been replaced in imperfectly-stopped bottles, were much softened and rapidly decomposing; anxious to save them, it struck me that by the addition of a small quantity of tannin to the spirit I should render the greater part of their bodies insoluble, and thus preserve them from destruction. I at once put my idea into practice, and the result seems to indicate exactly what I anticipated. Long experience alone can prove the value of this practice (which I presume to be new); but the theory appears to me to be so good that I am anxious to give the public the benefit of it, feeling how often I have myself had to regret the loss of valuable specimens which have dissolved away in spirit, and which I believe I could have saved had the use of tannin before suggested itself. I have the honour to be, Sir, your obedient servant,

ROBERT BALL.

Dublin, Oct. 4, 1838.

MEDICAL SOCIETY OF LONDON.

Monday, October 22nd, 1838.

Mr. BRYANT, President.

DISCUSSION ON THE CAUSES OF SEA-SICKNESS.

DR. J. JOHNSON said, that having had sixteen years' experience respecting sea-sickness, he would offer an opinion on its cause. He had never gone to sea, even although he had only been a fortnight on shore, without being sick, if there was any breeze at all. He had long come to the conclusion that the sea, *per se*, had nothing to do with the matter, but that the morbid effects were entirely owing to the motion. They were produced partly through the eye and partly through the body generally The eye was not the only medium through which the impression was made, for persons were sick, though not to the same extent, when in bed with the eyes closed, showing that another cause was in action. During all great earthquakes, as that of Lisbon, *sea*-sickness was very prevalent, arising from the agitation of the body and the sight of objects reeling. He was quite convinced that the impression was first made upon the brain

and nervous system; for, before the nausea came on he invariably felt a giddiness and unsteadiness about the head, indicating derangement of the sensorium; the next impression was on the circulation; before vomiting he noticed the face to be pale and the pulse small. The vomiting was an effort of nature to restore the equilibrium of the circulation and sensation. In proof that the agitation of the body was the chief cause, it was known that those who reposed in a well-slung cot, in which the agitation was considerably diminished, suffered far less than those who remained standing. The only *palliative* of sea-sickness was the horizontal position in the open air, and the bandaging of the eyes. He was in the habit of getting into the main or mizen rigging and covering his eyes. Ladies should go to bed, keep their eyes shut, and their stomachs empty,

Mr. ROBARTS thought no satisfactory explanation had yet been given of the cause of the sickness. An idea on the subject had occurred to his mind. It was known that certain sets of muscles act simultaneously; that, for instance, the abdominal muscles, the diaphragm, and the muscles of the pharynx, were all concerned in the action of vomiting. To irritate any of those various muscles would be sufficient to throw them all into action; thus tickling the fauces produced vomiting. It appeared to him that when persons were at sea, there was a possibility that, in attempting to maintain their equilibrium during the rising and falling of the ship, the abdominal muscles were thrown forward and retracted, and that the mouth and pharynx were open, and that sickness, after a time, was produced by these causes.

Mr PILCHER believed, that the cause of sea-sickness was more likely to be dependant on that vibration of the brain* which was produced by the rolling of the vessel. The brain thus shaken, as in cases of slight concussion, was not able to perform its functions. The morbid impression was conveyed by the par vagum, not only to the stomach, but to the various other organs of the body, through the influence of the nervous system. When a bandage was placed round the eyes, or the person fixed his gaze firmly on some immoveable object, the brain was not so much shaken, the body itself being more fixed.

Dr. BENNETT had found in his own person, that the first impression was made upon the stomach, before any perceptible effect had been produced on the brain. He believed, however, that the brain was first affected. Regarding the palliative of sea-sickness, lying *across* the ship and pressure applied to the epigastrium, as recommended by Dr. Pemberton, he had found frequently, though not always, beneficial. In a gentleman he (Dr. B.) knew, the sight of the undulatory motion of a looking-glass produced the sensation of sea-sickness.

Dr. WHITING thought the view most accordant with fact was, that there was a primary cause in action in all cases; but that it required, in most instances, the occurrence of some secondary cause to affect the stomach and produce nausea and sickness. We found, for instance, that a person might be exposed to malaria and not become affected, but when some depressing cause came into operation, such as debility or want of food, the malaria took effect. In others, this secondary cause was not required. So, in sea-sickness, some became affected by merely seeing others sick, &c. He believed that the primary cause depended on the motion of the body up and down. But how did this motion produce sea-sickness? The first action of this motion was on the circulation. He had found his brain perfectly free until the occurrence of the vomiting. When the ship pitched he had felt a sensation about the epigastrium, which feeling he considered did not lie in the stomach itself, but in the large vessels which run along the trunk. By this rising and falling of the circulating fluid, the blood was forced upon the brain, and left it as suddenly, and hence the production of the nausea and other effects.

Mr. CLIFTON believed that the morbid impression was first made on the nervous system, perhaps through the medium of the eye. He knew an instance in which two gentlemen, who were experienced voyagers, and laughed at sea-sickness, became affected by watching the oscillations of a lamp in the cabin.

After a few words from Mr. HEADLAND and other members, the Society adjourned.

* This is the theory of Baron Larrey, who also gravely assures us, that if persons inhabiting the sea-coast be less liable to sea-sickness than those dwelling inland, it is because the former have *less* brains to be *shaken* than the latter.—ED L.

WESTMINSTER MEDICAL SOCIETY.

October 20th, 1838.

THE Society held its first meeting for the session this evening; there was not, by any means, a full attendance of members. The President, Dr. WILLIAMS, made a few introductory remarks, in the course of which he stated, that the Committee had made arrangements by which the place of meeting would either be removed, or the room at present occupied by the Society rendered more comfortable, in order that it might not be liable to the same objections which were so justly raised against it last session.

The evening, as is usual at the first meeting for the session, was occupied chiefly

with the nomination of members for election as officers. A desultory conversation followed this proceeding, which offered nothing which we can make available for a report.

BRITISH MEDICAL ASSOCIATION.

The following is a correct list of the office-bearers of the British Medical Association for the year 1838-9 :—

President.—George Webster, M.D.

Vice-Presidents.

R. Davidson, Esq.	M. W. Hilles, Esq.
A. B. Granville, M.D.	J. Howell, Esq.
Marshall Hall, M.D.	George Pilcher, Esq.

Honorary Secretaries.

M. F. Wagstaffe, Esq.
C. H. Rogers Harrisson, Esq.

Treasurer.—Edward Evans, Esq.

Other Members of Council.

J. N. Bainbridge, Esq.	William Hughes, Esq.
J. S. Bayfield, Esq.	R. L. Hooper, Esq.
J. M. Beane, Esq.	George Hull, M.D.
Charles Brady, Esq.	John L. I'on, Esq.
J. S. Bristowe, Esq.	G. C. Ireland, Esq.
John Clarke, Esq.	John Lavies, Esq.
Edward Crisp, Esq.	Robert Liston, Esq.
Nathan Delph, Esq.	D. M'Ewan, M.D.
William Eales, Esq.	George Parsons, Esq.
William Farr, Esq.	Thomas Porter, Esq.
Profess. Grant, M.D.	Samuel Procter, Esq.
P. Hennis Green, M.B.	Robert Smales, Esq.
William Herring, Esq.	T. Wakley, Esq., M.P.

Honorary Solicitor.—David Davies, Esq.

UNIVERSITY COLLEGE HOSPITAL.

DISEASE OF THE SHOULDER-JOINT.—EXCISION OF THE HEAD OF THE HUMERUS.

W. E., aged 34, a butcher, was admitted, May 10th, under the care of Mr. Liston. He has been accustomed to carry his tray upon his right shoulder. He came to the hospital first in consequence of an abscess situated very superficially over the acromion process, on the right side; the abscess was opened, but did not heal up. Other abscesses formed around the joint, and were opened from time to time, until numerous sinuses ran all round the articulation. The man's health suffered very much during this period; he has also been subjected to an attack of erysipelas in the arm, which was followed by abscesses of the subcutaneous cellular tissue.

July 11. Was made an out-patient on account of his health, which is very much weakened.

August 3. He was readmitted to-day. Soon after his admission Mr. Liston, after a close examination of the affected parts, came to the conclusion, that the head of the humerus was diseased, and to the determination, as soon as the man's health would permit it, to remove the diseased portion by excision; in the mean time a leather splint was applied, with the view of keeping the joint at rest, and attempting to procure an anchylosis. He complained very much of the pain produced by the splint, and said he could not endure it; it was accordingly removed.

October 3. There is a sore on the inner side of the elbow, at the bottom of which the bone is detected bare by the probe; but Mr. Liston is inclined to think the affection is not extensive nor contra-indicatory of the operation which he intends to perform.

Operation.—With a bistoury a long incision was made from over the acromion, downwards, for three or four inches, between the fibres of the deltoid; another was made to join this in a transverse direction, over the outer surface of the joint; the angle of the flap was next dissected up, and the capsule of the joint cleared; the flaps were held aside by assistants by means of copper spatulæ; the elbow was then carried forwards and across the chest, so as to push the head of the bone against the portion of the capsular ligament that had been exposed, which was then divided, and the head of the humerus being turned out, it was sawn off a little below the anatomical neck; the cartilaginous surface had been removed by absorption; the acromial process was next removed with a portion of the clavicle; very little blood was lost; the glenoid cavity appeared to be sound; the vessels which bled did not require ligature. Lint was stuffed into the cavity; the edges of the lower part of the wound were brought together by a suture.

October 9. Has been doing exceedingly well since the operation.

16. Doing well; sits up by the fire.

21. Not a bad symptom has occurred; the operation will, no doubt, be a successful one.

WOUND OF THE HAND.—INFLAMMATION OF THE ABSORBENTS.

W. C., aged 45, was admitted, September 24, under the care of Mr. Cooper. About a month ago he wounded his left thumb slightly, but the wound did not heal; its edges became hard and callous, and he suffered from pain in the arm and the axilla, where one of the glands became enlarged and tender; the edges of the sore were cut away, and it soon healed; but in a few days after the general indisposition increased. He has been obliged to keep his bed the last three or four days. His wife has considered that her husband's health has not been so robust of late as it used to be, but she says that his habits of living are regular and moderate. On admission the man was depressed; pulse 110, full, but not strong; great tremor of the face; general clammi-

ness of the surface; loss of appetite; tongue coated and moist; restless at night, when he also wanders in his intellect; there are large elevated red patches upon the surface of the forearms and legs of each side of the body; tenderness of the epigastrium under pressure. A calomel pill, followed by a black draught; two grains of tartar emetic, six drachms of tartarised soda, with eight ounces of water, were made into a mixture, of which he took an ounce every two hours.

Sept. 25. Symptoms are more strongly developed than yesterday; prostration of strength greater; tongue more coated, but still moist; the erysipelatous patches are more extended and more vivid in colour; pulse 116, not strong nor weak; some sonorous and sub-crepitant râles in various regions of the chest. Continue medicines. To have beef-tea. A dose of muriate of morphia at night.

26. He is not so well again. Pulse frequent and voluminous, but very easily compressed; erythema more extensive; bowels open; tongue coated; copious perspirations; incoherency at night; prostration of strength and tremors of the muscles of the face still continue.

27. Pulse this morning is more feeble than yesterday, but not particularly so. Tongue is loaded, but not dry; external moisture of the surface, so that his linen requires frequent changing; tremor of the muscles of the face, and general anxiety of manner, still continue; redness less vivid externally; very delirious during the latter part of the night. Altogether he appears to be weaker than yesterday.

Three o'clock. Mr. Lewis, the house-surgeon, was called to him, on account of his restlessness. He was pale, anxious, and hurried in his manner; not capable of attending to what passed around him; universal tremor of the whole body and jactitation, especially of the hands and arms. Died at half-past four.

Autopsy.—Little that could explain the symptoms which had occurred during life was found within the great cavities of the body. Dr. Carswell drew the attention of the student to the circumstance, that he could, at pleasure, produce in the stomach all the vascularity of intense inflammation, by simply pressing with the fingers the conts of the viscus. He said that many, in his opinion, had deceived themselves and others by neglecting this phenomenon, for even a slight degree of pressure, such as might be used in taking out the stomach, would be sufficient to produce it. On opening the elbow-joint and right knee-joint, a large quantity of pus was found in each, though the synovial and cartilaginous surfaces appeared to present little alteration. No alteration was observed in the axillary vein, and, except swelling, none in the enlarged gland of the axilla.

IMPERFECT URETHRA—PHYMOSIS—OPERATION.

S. H., aged 21, was admitted Aug. 14, under the care of Mr. Liston. There is congenital phymosis, with difficulty in passing the urine, which comes away in a very small stream, and requires much exertion to force it away; has always suffered from pain and swelling of the prepuce after coition. At present the penis is as thick as a man's wrist, which is mainly owing to the hypertrophied state of the prepuce, which is very much enlarged, apparently from solid œdema. The glans penis can be discovered; slight discharge from the extremity of the organ. The prepuce was slit up by Mr. Liston; the section was made upon the dorsal surface. An immense raw surface resulted from the division of the skin from the mucous membrane. *A colocynth and calomel pill* at bedtime.

Sept. 7. On examination, the orifice of the urethra can hardly be discovered, and it is so much contracted that a small probe can hardly be forced into the canal. The contraction was found to extend at least an inch, or an inch and a half, from the external meatus. Mr. Liston introduced a probe-pointed bistonry, and enlarged its canal by cutting along its inferior surface. A large-sized catheter, introduced every other day, to maintain the dilatation.

20. Prepuce still much too large; the wound from the operation is much diminished by contraction; the urethra requires to be dilated frequently by the catheter. No. 8 is the largest that can at present pass the external orifice.

25. Has suffered of late from pain in his right loin, and passes a large quantity of thick ropy mucus. This is probably owing to the irritation produced by dilating the contracted portion of the urethra with catheters.

27. Is better; the œdema of the prepuce continues.

Oct. 9. Makes his urine freely; but the prepuce is still much too large and heavy. Discharged for misconduct.

ST. GEORGE'S HOSPITAL.

DROPSY, GENERAL AND LOCAL.

DROPSY (ASCITES) AFTER SCARLATINA.—Andrew Shanks, aged 2½ years, was admitted under the care of Dr. Seymour.

June 14. The history given to the nurse by the parents, perhaps scarcely to be relied on, is, that about nine months ago the child had scarlatina, and about one month after the appearance of the efflorescence, the first symptoms of ascitic distension of the abdomen appeared. Since that time paracentesis has been performed. The abdomen is now distended, but not to its fullest extent. There is also anasarca of the eyes and feet. The

urine is small in quantity and much loaded with deposits; the bowels are reported to be open. Dr. Seymour ordered him to drink half a pint of the *infusion of chimaphilla* in the course of each day. On the following day the bowels were still confined, for which he was ordered three grains of *calomel* at bedtime, and the following draught next morning:—

Tartrate of potash, two drachms;
Infusion of senna, one ounce and a half.

One drachm of *solution of potash* was also added to his daily medicine.

17. Flow of urine very scanty, less acid, it scarcely tinges litmus paper. Taking all the symptoms into consideration, he is decidedly better. Medicines to be continued.

19. The urine still scanty, but of a better colour; lips blue; countenance less puffy; hands cold; pulse very quick. Dr. Seymour prescribed—

Infusion of foxglove, two drachms;
Solution of oxymuriate of mercury, one drachm;
Tincture of Spanish fly, fifteen minims;
Water, one ounce. To be taken twice daily.
Syrup of white poppies, one drachm, every night at bedtime.

23. Urine slightly increased in quantity, and better in quality; bowels open; pulse regular. Ordered beef-tea for diet.

24. Much the same; has some troublesome cough, for which he was ordered—

Syrup of white poppies, half an ounce;
Simple oxymel, same quantity;
Fresh lemon-juice, two drachms. A tablespoonful when the cough is troublesome. The other medicines to be continued.

26. Urine the same, the medicine not having been given regularly.

28. Urine improved; the patient better; some cough remaining. Ordinary diet.

29. The infusion of foxglove to be omitted, and a small blister to be placed on the chest, followed by a poultice.

July 1. No prominent improvement. A *saline draught*, with 25 minims of *tincture of Spanish fly*, three times a day. The mouth being sore it was ordered to be rubbed with *borax* and *honey*.

3. The abdomen still considerably swollen, and there is considerable difficulty of breathing, without cough or expectoration. Ordered to be put in a warm-bath every other day.

5. His friends came to see him to-day and insisted on removing him from the hospital.

Dropsy after Scarlatina.—M. A. Shelstone, admitted under Dr. Seymour's care, June 21. Seven weeks since he had scarlet fever, to which succeeded effusion into the abdominal cavity, and general cellular tissue, three weeks after the fading of the efflorescence. There is swelling of the left leg and knee, with a sensation of fluid on pressure. His other symptoms not being severe, he was turned over to Sir B. Brodie, for the affection of the lower extremity.

Clinical Remarks.

On these cases Dr. Seymour remarked that, in general, dropsy did not supervene on the severer forms of scarlatina, but on the milder. In such cases the serous effusion came on between the sixteenth and twenty-first days of the disease, in one or more of the larger cavities of the body, or into the cells of the cellular tissue. Anasarca, he observed, is the most common form in which it shows itself; ascites the next so. The best and most appropriate mode of treatment is by bleeding, purging, and diuretics; these, however, will not always produce a cure after the acute inflammatory stage has subsided, and the chronic has been set up. Dr. Seymour differs from the opinion that the pathology of dropsy supervening upon scarlatina occurs in the condition of the kidney described by Dr. Bright. Dr. Seymour considers that the effusion following scarlatina is frequently caused by the neglect of free purgation during the eruptive disease, as it is frequently attended by diarrhœa. He believes that purging and cold external applications are simple, yet most valuable remedies in scarlatina. With reference to this disease more particularly, Dr. Seymour observed, that it shows itself by a ruby-coloured efflorescence on the skin, after two days previous fever, accompanied by a sore-throat, with either a number of small pustules, each forming a small slough, or with a more diffused redness and sloughing. Occasionally, but rarely, a single ash-coloured ulcer is seen in each tonsil, accompanied by a very fœtid breath. The heat of skin in scarlatina is higher than in any other known form of cutaneous disease, the thermometer, placed in the axilla, frequently rising from 98°, the natural temperature, to 104°. In three or four days the eruption declines, and desquamation of large pieces of cuticle takes place. As the eruption goes off an inflammatory action is sometimes set up in some of the viscera, affecting particularly the serous membranes covering them. Dr. Seymour has seen this inflammation and consequent effusion, in the cavities of the brain, the heart, and the lungs, but never in the abdomen; he believes the brain to be the most common seat of the affection, and that many persons die after this disease from effusion into the ventricles.

This peculiar inflammation (the lecturer continued) is of a somewhat different character from the supervening dropsical effusion after scarlatina: the former of these comes on usually as the scarlet eruption declines; the latter not until a much later period. The treatment of scarlatina in England and on the Continent, is very different. In this country we repress the heat and

inflammatory condition of the skin by cold air and cold water, and treat the throat with cooling gargles. In the south of Europe, on the contrary, every precaution is taken to keep the patient from the access of any cold air; he is plastered and swaddled, and gets nothing to drink but ptisans. The consequence is the more common occurrence of dropsy after scarlatina out of England than in it. Every eruptive disease (more especially measles and scarlet fever) will, generally, if left to itself, terminate by diarrhœa, and if this do not come on, dropsy generally supervenes, to prevent which it is always advisable to purge well through the course of the fever, and afterwards also. Following this practice Dr. Seymour says he has not seen above one case in fifty of dropsy occurring after scarlatina.

In these cases of dropsy the urine is scanty in quantity, and in appearance resembles water in which flesh has been boiled; and sometimes it coagulates. Dr. Wells mentions three cases out of five in which this latter symptom predominated, but the urine contained globules of blood, and he cured those patients by cantharides; so that it was evident that he did not consider the kidneys as over-stimulated. The supervention of dropsy, after scarlatina, is best prevented by the external use of cold water, and the internal exhibition of saline purgatives. When, however, the disease has made its appearance, the *infusum chimaphillæ cocymbosæ* is a very effective remedy; in the early stage of the disease bloodletting, also, according to the strength of the patient, is very useful. In addition to saline purgatives and diuretics, you will find the liquor potassæ of great advantage. Should any symptoms of effusion into the cavities of any vital organ, as the brain, for instance, come on, then the most prompt and energetic treatment will become necessary. Bleeding, blistering, and calomel every few hours, will then be prescribed, together with the use of diuretics and digitalis, as well as any other remedy that is calculated to check and remove the effusion. With respect to the case of Shanks, (Dr. Seymour added) you must have remarked, that it is one of long standing; the urine was increased in quantity, under the employment of digitalis, which was obliged to be discontinued, because the pulse became very small. My views of the pathology of the disease I have before mentioned to you.

Encysted Dropsy.—Ovarian Tumour.—Anne Beale, aged 31, admitted June 14th, under the care of Dr. Seymour. She is a widow, without "incumbrance," excepting the disease. About fifteen months since the abdomen began to swell, but the distension has been far more rapid within the last fifteen months. She complains very much of sickness and vomiting, apparently kept up by the pressure of the tumour. The catamenia are regular, the bowels are confined. One drachm of *mercurial liniment* to be rubbed over the abdomen every night and morning. The following enema at bedtime:—

Confection of rue, three drachms;
Infusion of chamomile, one pint.

17. Much the same. One drachm of *solution of potash*, in fresh small-beer, three times daily, and fifteen minims of *laudanum*, in a draught, at bedtime.

19. Complains of pain over the epigastric region, which appears to be produced by pressure; sleeps better since taking the anodyne draught; the sickness has gone off, and the urine has increased in quantity.

26. She still suffers from epigastric pain, extending round to the back; the abdominal swelling is somewhat diminished. Medicines to be continued. Ordinary diet.

July 5. The epigastric pain has greatly subsided; better in many respects; the medicines to be taken four times daily.

10. Slight return of pain to-day in the epigastric and hypochondriac regions, which Dr. Seymour attributes principally to pressure. General health otherwise very good. The tumour is softer. She says, that the abdominal distension increases, although the eye cannot detect it.

22. Since last report the bowels have been much confined, for which she has been obliged to take strong purgatives, and to omit the *solution of potash*. To-day, owing to the continued pain, she has had a blister over the epigastric region, and the following mixture has been ordered, to check flatulency:—

Fœtid spirit of ammonia, sixteen minims;
Camphor mixture, two ounces. To be taken when necessary.

24. Somewhat better; the distension has not perceptibly diminished; complains of her legs swelling frequently. To be bled to twelve ounces.

26. Blood neither buffed nor cupped; pulse 100; complains much of distension.

Iodide of potash, one drachm;
Prepared lard, half an ounce. One drachm of this ointment to be rubbed in, over the abdomen, every night and morning.

31. The pain over the hypochondriac region much relieved; the distension reported to be the same; the feet still swell. The injection of rue to be repeated whenever necessary.

Aug. 5. *Hyd. of potash*, three grains;
Iodine, ¼ of a grain;
Distilled water, ten drachms. To be taken twice daily.
Opium, one grain;
Extract of hemlock, seven grains; as a

suppository every night. The liniment to be rubbed in as before.

9. Less pain; urine slightly increased in quantity; in other respects the same. Ten grains of the *compound gamboge pill* at bedtime, the bowels being torpid.

13. The distension not having been lessened by the medicines, she was recommended to Sir B. Brodie as fit to be tapped.

14. Tapped yesterday; thirty pints of clear fluid drawn off, which coagulated strongly by heat. A considerable mass of tumour can be felt through the abdominal parietes on the right side; the flow of urine abundant; the pulse quiet; bowels not open; feels more comfortable. Half an ounce of *castor oil* directly, and a mutton chop for dinner. The operation having placed her beyond the pale of medicine, the future progress may be summed up by stating, that in a fortnight the fluid commenced re-collecting, and powerful medicines failing to remove it, and her constitution beginning to suffer in the hospital, she left soon afterwards as a "relieved" patient.

Clinical Remarks.

In remarking on the diagnosis of such cases Dr. Seymour observed, that it was a common opinion that the fluctuation of fluid in ovarian dropsy was less perceptible than in ascites; he, however, considered it easier to distinguish the former than the latter. Another aid towards forming a correct prognosis is, that in encysted dropsy the general health is not so extensively impaired as in ascites; in ascites both the legs swell; in ovarian dropsy, frequently, there is only one affected, corresponding with the side on which the ovary is diseased; in ovarian dropsy there is always some flatulent distension, owing to the tumour pressing on the bowels. In these cases it is of very great advantage to administer injections containing *confection of rue*, to assist the practitioner in forming a correct opinion as to whether the distension is really produced by the fluid. Some time since a patient, under the care of Mr. Lizars, of Edinburgh, was supposed to have ovarian tumour; an incision was made from the ensiform cartilage to the symphysis pubis, for the purpose of removing it; the bowels immediately protruded through the aperture, filled, and distended with gas. The case was one of hysterical tympanitis.* Ovarian dropsy seldom occurs simply and alone; there is usually a large tumour to be detected through the abdominal parietes, of varying size and consistence; this tumour is composed of different sized cells containing different fluids. If one single cell be so large that fluctuation in it is very evident, then the disease is properly termed "ovarian dropsy;" if the cysts be small, and there be no feeling of fluctuation communicated to the hand, the name of "ovarian tumour" is then more properly applied to it; but under each of these circumstances the disease is the same. There is a disease in which the ovary is the subject of very rapid growth; it consists, generally, of one large cell, with other small cells around it, and these contain, usually, a semi-solid substance, very much like birdlime. This is a very malignant disease, and is generally connected with symptoms of a similar character showing themselves in other parts of the body. The French have given the name of "cancer colloide" to this disease.

It is considered as a practical rule (continued Dr. Seymour) in this disease, that tapping is not to be performed until there exists a positive necessity for it. So great is the fear which some practitioners have of tapping in this disease, that I have known them to postpone it until sloughing of the abdominal parietes, from the pressure of the tumour, has taken place; and in other cases I have known the operation deferred until nature had made a fistulous orifice through the abdominal muscles for herself. I have seen drawn away at one tapping as much as a hundred pints in one instance, but the patient filled again in four years. A stimulating injection into the cyst of the tumour has been recommended, or a catheter to be kept in the orifice; but I do not known that either method has ever produced anything but severe inflammation. In a case now up stairs the patient was tapped, and a large quantity of fluid discharged, but the orifice remains fistulous, and fluid oozes out in considerable quantities. In such cases there is sometimes the danger of inflammation on the surface of the cyst ultimately proving fatal. Mr. Key, of Guy's Hospital, has sent me the notes of some cases which corroborate this opinion. The operation of excising the cyst has been recommended, and Mr. Lizars has written on the subject; but the first thing to ascertain is, whether the tumour adheres to any of the surrounding textures, which, in ninety-nine cases out of a hundred it does. But the difficulty is sometimes very great in detecting whether the tumour is really ovarian. I have known hysterical tympanitis and fæcal matter in the colon mistaken for an ovarian swelling.

As to the treatment of these cases, I have seen great benefit derived from the *solution of potash*, in drachm doses. I generally pre-

* In the wards one day Sir Benjamin Brodie saw a patient who had just come in with the abdomen enormously distended. He percussed the parietes, and pronounced it to be only a case of hysterical tympanitis; this opinion was fully verified, for, on the exhibition of fœtid purgatives, with a few injections containing confection of rue, the swelling entirely disappeared, and she left the hospital with a becoming shape and form.—Rep. L.

scribe it, and so does Sir Benjamin Brodie, to be taken in a glass of fresh small-beer; stale beer, from its acidity, will not do, and the bitter of the hop assists the stomach in retaining it. I give it in these large does because in cases of enlarged fatty glands it frequently diminishes them considerably.

A patient with ovarian tumour *may* recover. I have known sixty-six cases, of whom sixty died, and six recovered. This proportion is very small, but in chronic malignant disease it is not so very bad. I knew the wife of a medical man who recovered from ovarian tumour. She was bled, and had opium suppositories, took calomel and opium to combat inflammation, and began taking the solution of potash, in drachm doses, which were afterwards increased to one drachm and a half. She lay in bed for eight months, taking about six drachms of the solution daily. The preparations of iodine have been much recommended, but they sometimes produce severe secondary fever. A young girl had some scrofulous disease of the abdomen, and took two grains of iodine twice daily, the bowels during the time being kept gently open; but it left her nervous system in such a shattered state, and the slightest thing would so agitate her, that when I saw her I was puzzled how to treat her case. When this medicine was first introduced at Geneva, it used to cause abscesses in various parts, and sometimes destroy life. It has been said to produce even sudden death, but that I do not believe. The tincture of iodine acts more powerfully than the hydriodate of potash. Sir Charles Bell has recorded a case of spongy enlargement of the uterus, accompanied with prolapsus, where a ten-inch probe could with ease be introduced, and after using the hydriodate of potash, in drachm doses, three times a day, for some months, no trace of the tumour remained. In my own practice, however, I have found the solution of potash more beneficial than the hydriodate. In the case of Beale the solution has been of very evident service.

EIGHTEEN GRAINS OF BLUE-PILL FATAL IN AN ADULT.

To the Editor of THE LANCET.

SIR:—Observing that a correspondent of THE LANCET has lately met with some severe cases of salivation, proceeding from the administration of small doses of mercury, and having recently met with a fatal case myself, I think it may be of use to the profession to give the particulars, hoping that others who may meet, or have met, with the like cases, will also publish them, as I have reason to believe that salivation proves fatal much more frequently than is generally allowed.

Mrs. Conn, a married woman, aged 19, applied to me on the 29th of September, in consequence of pain in the right side. The eyes and skin being tinged with bile, I ordered her to take three grains of blue-pill twice daily for three days.

On the 3rd of October I visited her, and found her in a state of ptyalism, the bowels confined, profuse perspiration. The pain in the side had subsided. Ordered,

Sulphate of magnesia, two drachms;
Carb. of magnesia, half a drachm; twice daily.

4. Bowels relieved copiously; motions of a dark colour and very offensive; pupils of the eyes very much dilated; complains of intense thirst, but no pain. Ordered,

Solution of chloride of lime, two drachms;
Water, a pint; to be used as a gargle and lotion for the mouth.
Sulph. of quina, three grains;
Dil. sulph. acid, three drops;
Water, an ounce; to be taken three times a day.

5. Salivation much increased, ulceration of the gums had taken place; breath very offensive; pupils of the eyes much dilated. Continue the quinine and gargle.

6. Sent for early in the morning in consequence of hæmorrhage from the nose; blood black and tarry, and did not coagulate; pupils very much dilated; breath not so offensive as yesterday; bowels regular, motions of a good colour. The limbs (but more especially the arms) covered with purple spots, generally of the size of No. 1 shot, but many larger. Continue the medicine, with rich soup and brandy.

7. All the symptoms aggravated. The pupils extremely dilated, so much so, that the iris was hardly visible; the purple spots have enlarged; the gums have fallen from the teeth, and the teeth are loose; pulse 130, and intermittent.

Comp. Spirit of ammonia, thirty minims;
Water, one ounce; every second hour.
Brandy, soup, &c.

8. Appears to be sinking; pulse fluttering; great restlessness, with picking of the bed-clothes; difficulty of breathing; muscæ volitantes; cold clammy perspiration of sour odour. Continue brandy, ammonia, &c.

9. Died.

As the remedies I employed in this case, as well as in many others, seemed to have little control over the salivation, I shall feel obliged to any of your correspondents who will favour me with the result of their experience in such cases. I am, Sir, your humble servant

JOHN WILLIAM POTTER.

Chipping Ongar, Essex,
Oct. 12th, 1838.

SIR HENRY HALFORD AND MR. LOCKLEY.

To the Editor of THE LANCET.

SIR:—I think it worth while to inform you of a circumstance which, I believe, is unparalleled in the history of the medical profession:—About a fortnight since Mr. George Frederick Lockley, of Half-Moon-street, was invited by Sir Henry Halford to accompany him, for a day or two, to his house in the country, and Mr. Lockley accordingly accompanied Sir Henry on the Birmingham Railroad for that purpose. Shortly before the train arrived at Tring, Mr. Lockley was seized with a fit, which I believe to have been apoplectic. Sir Henry Halford had him conveyed to the waiting-room for passengers and told the person at the station that he was highly respectable, gave his (Lockley's) name and address,—desired a medical man to be sent for, and no expense to be spared, and then, will it be believed coolly took his seat on the same train and went home to dinner. It was six hours before a medical man could be found, and the first who saw him was a physician from London. Mr. Lockley died yesterday at his house in Half-Moon-street, aged 59.

October 15, 1838.

We received this letter last week, but were unwilling to publish, on the assertion of an anonymous writer, a fact which, for the sake of humanity, we still hope may admit of some explanation. As, however, the circumstance has been mentioned in the political journals, we do not think it necessary to withhold the communication any longer.—ED. L.

BOOKS RECEIVED.

Report on the Malignant Fever called the Pali Plague, &c., by James Ranken, M.D. Huttmann, Calcutta, 1838.

Traité de l'Affection Calculeuse, &c.; or, Treatise on Gravel and Stone; being Researches on the Formation, Characters, Causes, and Symptoms of Urinary Calculi; by M. Civiale. Crochard and Co., Paris, 1838. 8vo. pp. 712, with five plates.

A Practical Synopsis of Diseases of the Skin; by Richard Hunt, M.R.C.S. Longman and Co., London, 1838. 8vo. pp. 142. [This will be found to be a useful manual of diseases of the skin, but might have been rendered still more useful if the author had been less concise.]

Anatomical Tables, containing concise Descriptions of the Muscles, Ligaments, Fasciæ, Blood-vessels, and Nerves; by Thomas Nunneley, Lecturer on Anatomy and Physiology, in the Leeds School of Medicine, &c. Highley, London, 1838. 8vo. pp. 240.

[An extremely *concise* description, indeed, being little more than a catalogue of names. The book, however, will answer extremely well for the purpose of "grinding," for the promotion of which it is apparently written.]

The Surgical Anatomy of the Perinæum; by Thomas Morton. Illustrated with lithographic plates and wood engravings. Taylor and Walton, London, 1838. 8vo. pp. 80.

Outlines of Human Osteology; by F. O. Ward. Renshaw, London, 1838. 12mo. pp. 214.

[This is the best work of the kind and form which has been published for many years; we can safely recommend it to the student.]

Urinary Diseases and their Treatment; by Robert Willis, M.D. Sherwood and Co., London, 1838. 8vo. pp. 408.

The Principles of Surgery; by John Burns, M.D. Longman and Co., London, 1838. 2 vols. 8vo. pp. 1084.

Practical and Surgical Anatomy; by W. J. Wilson. Illustrated with fifty engravings on wood. Longman and Co., London, 1838. 8vo. pp. 492.

TO CORRESPONDENTS.

WE cannot undertake to give publicity to any communications on *Animal Magnetism*. Letters of condolence, &c. may be addressed to the *Middlesex Owl*, who will also thankfully receive any "necessaries," &c. for his bantling, which, by this time, must be *in extremis*, if not actually dead.

The length of our report of the trial of Sir A. Carlisle, compels us to defer, until next week, a notice of Dr. Macartney's work on Inflammation; as, also, an *original* Memoir of *John Hunter*, which has never before been published.

The letters of *Mr. Bedingfield*, *Mr. Ellis*, *Dr. Wilmot*, a correspondent near Worcester, *S.*, a subscriber to the Nottingham Reading-rooms, the Medical Officers of the Nottingham Lunatic Asylum; and an inquirer respecting dentists, *Dr. Furnivall*, *Dr. Willis*, *Mr. J. Millar*, *Mr. P. T. Chambers*, and *Dr. Hancock*, have been received.

W. R. There is no other means than that of passing the Hall.

A correspondent observes, with reference to the letter of Mr. Marshall, in our last Number, that Mr. M. does not contradict the statement in THE LANCET, but gives another, though a very different version of it. With reference to that part of his letter, our correspondent remarks, that it need only be mentioned, in justification of the statement in this Journal, that the fact respecting the "Doctor's" rejection of a "general practitioner," as his coadjutor, was given on Mr. Marshall's own authority. It is possible that Mr. M. was misunderstood, though his simultaneous condemnation of the "Doctor's" dislike of "general practitioners" was honestly given in terms not at all equivocal or obscure.